A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY 1799.

ARTICLE I.

Description of a new Instrument for drawing equidistant and other parallel Lines with great accuracy and expedition; intended principally for the use of Engravers. With Specimens of its Performance. By W. N.

SOME months ago I was informed by the Engraver who executes the plates for this Journal, that an ingenious artist had constructed a machine for ruling the shades and grounds of copper-plate engravings, which he not only used with great success in his own business, but had sold to others under an engagement of secrecy. How far this information might be strictly accurate, was of less consequence at the time this conversation took place, than the consideration that it would be easy to make such a tool, and render its advantages more general by publication. I then promised to make one; and having now finished it, I take the earliest opportunity of communicating it to the world. From the preceding historical statement, the reader will see that I have no claim to the original thought of substituting mechanical operation instead of hand work in this department of engraving; and those who have seen the screw gear of Ramsden's great dividing engine will also perceive, that I have done little more than distribute the parts of this tool in what appeared to me to be the most simple and convenient manner.

Fig. 1. Plate XX. represents the instrument of the size of that which I have made. The outline section, fig. 2, represents the same viewed in a direction parallel to the edge of the moveable ruler. The letters denote the same things in both. A A A A is a frame fixed to the drawing-board. It resembles that of a sliding rule, and serves to guide a sliding piece, which, lying in fig. 1, immediately under the screw, could not with convenience be denoted by any letter. B C is a screw of exactly forty threads in the inch, but might conveniently be made of a coarser thread. G H and E D are two cocks, the former of which, G H, is fixed to the frame A, and bears a clip or pair of nuts, which open and shut with a joint like a pair of compasses, and either embrace the screw by a re-

 gularly

gularly tapped part when ſhut, or leave it at liberty when open. Fig. 3. ſhews the face of this cock with the clip, of which P is the joint, Q Q tapped part, R a pin to inſure their coming up fairly, and S a claw, which, when drawn upwards, ſerves to cloſe them; or, if thrown back, leaves them at liberty to open by the action of a ſmall ſpiral ſpring, let into oppoſite holes near the joint. E D, fig. 1, repreſents the other cock, which is fixed to the ſliding piece. It carries the ſteel ruler E F, which, though ſufficiently ſtrong, is thin enough to adapt itſelf to ſlight variations of thickneſs of the plate beneath it. In my inſtrument I have made it adjuſtable to much greater variations of thickneſs, by means of an horizontal axis; but as this contrivance adds to the expence, and diminiſhes the ſimplicity of the inſtrument, I would rather recommend that great variations ſhould be allowed for by putting paper or thin ſlips of metal underneath the plate A A, as may be required. The end C of the ſcrew is turned down, and fixed in the cock E D by means of a nut and waſher; or in any other of the methods which are familiar to inſtrument-makers. The upper part of the cock E D is filed round, and cut into teeth, of which fifty would complete the whole circle. The centre of this external circular part correſponds with the axis of the ſcrew (ſee fig. 4). L, M repreſent two ſhort cylindrical pieces which are hollow, and apply to each other ſo as to form a kind of box. Within, and fixed to the part M, which is fixed to the ſcrew itſelf, there is a ratchet wheel divided into fifty teeth; and within the part L, which is merely ſupported by the cylindrical part or ſtem of the ſcrew, there is a ratchet, which holds when the part L is moved by its handle N from right to left, but eſcapes when that handle is moved in the contrary direction. O is a lever, or arm, likewiſe ſupported by the ſtem of the ſcrew, and occupying the remaining ſpace between the handle N and the cock D. At the outer extremity of this lever there is a ſmall ſteel blade, which, by means of a back ſpring exactly reſembling that of a pocket knife, may be made to form a continuation of the lever itſelf, or, by being placed at right angles to the lever, may be made to reſt in any of the diviſions between the teeth of the circumference of the cock; and conſequently will, by that means, confine the lever to the poſition in which it is placed. The handle N cannot paſs the lever O, becauſe this laſt is too thick, and there is a ſtud or pin T, fig. 4, upon the face of the cock D, which prevents the handle from being moved beyond a certain determinate ſtation to the left hand. And, laſtly, the ruler I K, which is repreſented as broken off at K, but (like E F) may be of any required length, ſerves, by means of a thumb ſcrew at I, and another at the oppoſite end, to ſecure the copper-plate againſt the drawing-board in the uſual manner.

After this deſcription the uſe of the apparatus may be eaſily underſtood. By drawing back the claw S, fig. 3. the ſcrew is ſet at liberty, and the ruler E F may be brought to any required diſtance from I K by hand. The plate may then be duly placed and ſecured to the board, and the clip drawn gently together by the claw S. In this ſituation, ſuppoſe the lever O to be placed at a conſiderable diſtance from the handle N, that handle may be moved to the right, during which the click will gather upon the ratchet wheel; and then being returned to the left, it will carry the ſcrew round. The gentle preſſure, exerted by means of the claw, will tend to cloſe the clip upon the ſcrew, as ſoon as it comes into a fair poſition by its rotation; at which inſtant the claw will ſuddenly fall into its place, and the machine is ready for work, excepting that the adjuſtment for the fineneſs or coarſeneſs of the

the ſtroke muſt firſt be made. This is done by the lever O. If the ſteel blade be dropped into the firſt notch beginning on the left hand, the handle N will be confined; if at the ſecond notch, the handle, upon being moved backwards and forwards between the pin T and the lever O, will move the ſcrew through one tooth, or one-fiftieth of a turn, each time, and conſequently will carry the ruler F through one 2000th part of an inch. If the blade of the lever O be placed in any other of the notches (which are numbered by filing the corners of every fifth notch), the quantity paſſed over, at each return of the handle, will be greater or leſs according to the number. As there are but twenty-ſix notches, the greateſt ſingle ſhift of this inſtrument will be one-eightieth part of an inch; but as the ſhift is ſo readily made, it is eaſy, even with this fine ſcrew, to reach greater intervals, by moving the handle once, twice, or even three times, between ſtroke and ſtroke. Thus for one-fiftieth of an inch, or $\frac{40}{2000}$ths, the number of intervals cannot be paſſed over at one ſtroke; but, if the blade be ſet at the twentieth notch, the ruler will be ſhifted exactly that quantity by two movements of the handle. It is needleſs to multiply directions for this part of the operation; and the more ſo, as the artiſt is much more likely to determine the fineneſs of his ſhade by previous trials, which will render him acquainted with the inſtrument, than by any numerical computation.

An inſtrument like the preſent may alſo be uſefully employed in ſubdividing lines into ſmall parts. If curved lines be required to be drawn at equal diſtances from each other, the ſimpleſt method appears to be that of varying the figure of a temporary edge of horn or metal, which may be fixed to E F: and if lines be required converging to a point, the ruler E F may be made to occupy the radius of a circle of any magnitude whatever, by giving an adjuſtable angular figure to the ſlider, and cauſing it to move againſt two points, upon principles deduced from the thirty-firſt propoſition of Euclid's third book.

As this inſtrument was made at intervals of leiſure, without any account being kept of time or materials, I cannot diſtinctly ſtate the coſt: but I eſtimate that the whole being made of braſs and ſteel, and well finiſhed, with a ſmall drawing-board and the wooden holding piece I K, might be afforded by a ſhopkeeper in the mathematical-inſtrument buſineſs for ten guineas. If the frame and ſlider were of box-wood, which I have no doubt would anſwer very well, and the ornamental covering L M of the ratchet-wheel were omitted, I ſuppoſe the inſtrument could be afforded for conſiderably leſs than half that ſum.

The engraver has uſed this apparatus in the plates of the preſent number.

II.

Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, *of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M. R. I. A. Correſponding Member of the Royal Society of Edinburgh, &c.*

[Concluded from Page 386, Vol. II.]

Of the Effects of theſe Weſtern Tempeſts on the General Temperature of the Climate.

IN a paper, honoured with a place in the ſecond volume of the Tranſactions of the Academy, I recorded the reſult of ſome experiments made in the year 1788, for the purpoſe of aſcertaining the temperature of the earth in our latitudes.

From these experiments it appears, that the medium temperature of the kingdom of Ireland, from its southern to its northern extremity, nearly coincides with the standard assigned to corresponding degrees of latitude in Mr. Kirwan's ingenious tables *.

It is further discoverable, that the general warmth of our island is in no respect inferior to that of other countries in its parallel: from whence we are naturally led to conclude, that the annual quantity of heat received here is not less in our day than it was in former ages.

If these things are so, it may be denied that there is any substantial foundation for the frequent complaint of change in our seasons. The ocean may be agitated by storms, sands be drifted along our coasts, and trees blasted in mountainous and exposed situations; but why should the farmer lament the diminished ardour and shortened period of his summers, as if winter possessed more than its natural portion of the revolving year? Whence should arise the gardener's complaints, that his peaches, however sheltered from storms, rarely now arrive at maturity; and that his May-duke cherries, even in the most favoured aspects, no longer merit that distinguished title?

May I be permitted to assume, as true, the fact which it has hitherto been the purpose of this paper to demonstrate, and in support of which the trees, the sands, and the tides of our island seem to bear constant and unequivocal testimony; namely that, of late years, the western winds have blown with increased violence? From that single principle an answer may be deduced to all these interesting questions.

It is a matter well known, and easily to be explained, that the surface of the ocean varies less from the mean annual temperature of its latitude, than land in the same parallel †: that is, the surface of the sea is colder in summer, and warmer in winter, than the surface of the ground in that latitude.

If then the prevalent winds of any country blow over an ocean situated in its parallel, that country will relatively be denominated temperate; it will be free from all extremes; the heats of summer and the colds of winter will be checked by sea breezes of a contrary property; and the land, influenced by the neighbouring element, must, more or less, partake in its equability of temperature.

Such is the case in almost all the islands of the world: and such from immemorial ages has been the peculiar character of Ireland. Hither come the western winds, modified by the temperature of the broad Atlantic ocean, which they traverse in their career: hither fly the clouds, teeming with moisture collected in a course of three thousand miles along its surface; and here uniformity of temperature, and redundant humidity, have always been marked as the distinguished character of our soil.

Since then the trees, the sands, and the tides demonstrate that these winds have, of late years, blown with unusual violence; since they bear testimony that a large quantity of air thus directed, thus tempered, and surcharged, has passed over our lands; it plainly follows that the climate must have felt the change; that it must have experienced colder summers and milder winters than heretofore, approaching towards that equability of heat and redundance of moisture which the farmer and the gardener at present so heavily lament.

But it is not from increase of quantity alone that these winds have produced their effects.

* See Kirwan's Estimate of Temperatures. † Ibid.

They

They have altered the temperature of the ocean itself; and thus have, as it were, multiplied their changeful influence on the land; acting there at once with new properties, as well as with increased quantity.

The surface of water in a state of tranquillity admits of greater variations of temperature than in a state of agitation. It may become much hotter in summer and colder in winter when calm than when disturbed; for the particles at the surface, when heated or cooled, do not immediately give place to others nearer the bottom; the process of commixture, in a tranquil state, is gradual, and the transmission of change somewhat resembles the slow and retarded progress that takes place on the land. Agitation always counteracts this gradual process; a rapid commixture of the particles produces a quick assimilation of temperature throughout the whole mass, and, thus taking away all partial excess, reduces the whole toward a medium state of uniformity.

Thus, summer tempests always tend to diminish the superficial heat of an ocean over which they rush; while winter blasts, agitating the waters at considerable depths, resist the natural cold of the season by a supply of relatively warm particles, which arise from the bottom toward the surface.

That the Atlantic ocean, the vast and potent arbiter of our seasons, has suffered unusual agitation of late years, seems evident from the natural phenomena recorded in the beginning of this Memoir. The trees and sands of our island bear testimony to the storms which sweep along its surface; and the tides come to us as frequent and unerring messengers of the tumultuous and agitated state of its billows attendant on their furious career.

From this perturbed abyss of waters has arisen an unceasing influence, equally potent to check the ardour of the summer solar beams, or to relax the shackles of a northern winter. By this prevailing influence, summer has been rendered impotent to raise and ripen many of our crops; and the farmer, taught by necessity, learns now to hand them over ready grown, and prepared for the maturity of summer under the mild temperature of an Atlantic winter. Hence too the gardener has, of late years, been compelled to call in the aid of artificial heat to forward the peach, the nectarine, the grape, and every other species of delicate fruit to perfection.

Hence premiums now vainly attempt the growth of hemp, which formerly peasants, in the most northern counties of Ireland, cultivated without reward around their cabins for domestic use. And hence the ancient apiaries of our island, once so celebrated, and guarded with such special and minute attention by the Brehon laws *, are now extinct; and honey, from being a common article of popular consumption, has become a rare luxury, or an expensive medicine.

Winter has likewise felt the general influence of this Atlantic temperature; our grasses scarcely droop beneath the frosts; wheat and oats vegetate in the open fields during the very solstice itself; myrtles and laurels, in sheltered situations, brave the severity of winter; the Foyle, and other large rivers of the northern province, frequently subject to the icy chains of former ages, now run in uninterrupted freedom †.

* See Collectanea de Reb. Hib. Transl. Brehon Laws.

† Fifty years have elapsed since the river Foyle has been effectually frozen over at Derry. It is also observed that the river Thames, in Britain, is less frequently frozen of late years than formerly. See Archæologia Britannica, vol. iii. p. 55.

Facts,

Facts such as these are to be considered as the basis of general opinion concerning the alteration of our climate; while old age, connecting these appearances with the fragility of declining life, and a decayed constitution, has become querulous in proportion to its feelings, and, judging of external phenomena by the exaggerated test of its own acute sensations, emphatically pronounces that the seasons are now less favourable than formerly.

On the Consequences of this Equability of Temperature to Animal and Vegetable Life.

FROM the transient perusal of this Memoir, a rapid and impatient mind would probably draw numberless false conclusions. The facts here recorded seem to wear a gloomy aspect; to mark a gradual deterioration of our seasons; to indicate a climate, harsh, ungenial, and of consequence sterile in its nature; clouded, humid, tempestuous, cheerless, and unfriendly to animal and vegetable life.

All these conclusions contradict experience, and may be overturned by a calm consideration of the phenomena themselves.

Experience teaches us that dry seasons and easterly gales are, in our island, invariable sources of feeble vegetation and numerous diseases*; and the history of the world informs us that winds, whether hot or cold, are in their nature deleterious to animal and vegetable life, in proportion as they become deprived of humidity. The warm air of the African desert breathes desolation over the parched land of Egypt and Syria, before it is yet felt as a tempest †. Even our own westerly winds, the Atlantic messengers of health and fertility to our island, after traversing the cold and dry tract of the vast Siberian continent, bear nothing but sterility, and almost perpetual winter, to the unhappy climate of Kamschatka ‡.

Heat or cold in extremes, dry air in rapid motion, and moist air in a stagnant state, seem to be the principal external sources of human diseases; and climates are generally found favourable to health and longevity in proportion as they are exempt from these natural causes of disorder and decay.

It ought therefore to be inferred, *a priori*, that Ireland, celebrated for the singular equability of its temperature, and the ceaseless motion of an atmosphere always influenced by the moisture of the Atlantic, should be likewise characteristically free from natural disease; and experience proves that this conclusion is true.

The exhausting agues of North America, or the fens of England §; the fatal fluxes which prevail in the low countries of the continent of Europe; the dreadful bilious distempers of both the Indies; the pestilence which desolates the African and Asiatic climates,

* Of this the influenza, attendant on the easterly winds of spring in the present year (1795), affords a strong instance.

† See Volney's account of Egypt and Syria.

‡ The latitude of Kamschatka corresponds with that of Ireland; the westerly winds are prevalent in each; yet the former experiences a rigorous winter of nine months, and the latter rarely of as many days. See Cook's Voyage in 1779, vol. iii. ch. 6.

§ The ague is so rare in several parts of Ireland, that many persons are totally unacquainted with it. In the northern province the author has never met with the disease, and its existence there is generally denied.

mates, are all either entirely unknown, or but feebly felt in Ireland. There is here no characteriſtic diſeaſe to mark the natural ſource of unhealthfulneſs. There are few diſorders which cannot be directly traced up to ſome artificial cauſe; to ſome intemperance, to ſome neglect, to ſome exceſs, either of luxury or penury, in the ſuffering ſubject. Manufactures in their kind unwholeſome, induſtry exerted beyond its proper limits, irregularity of food or fermented liquors, illicit amours, colds, the conſequence of folly or inattention, exceſs or deficiency of proper exerciſe, anxiety and fretfulneſs of mind, together with the copious liſt of caſualties, afford to the phyſician of this country the cauſes and explanation of almoſt all its diſorders.

From its peculiar ſalubrity, the natives of this iſland are celebrated through Europe for juſt ſymmetry of proportion and an athletic frame; becauſe, from earlieſt infancy to manhood, a check is rarely given to the progreſſive increaſe of animal ſtrength, or the natural and appropriate forms of an undiſeaſed body. From the ſame hygeian ſource flow thoſe ardent paſſions, thoſe exuberant ſtreams of animal ſpirits, which render our natives always cheerful, oftentimes turbulent and boiſterous, the uſual conſequences of uninterrupted health and a vigorous conſtitution. Hence wild adventure, perſonal courage, impetuoſity of purſuit, inattention to conſequences, and improvidence of diſpoſition, become the characteriſtic features of minds ſeldom under the influence of that anxiety, which, flying from the preſent moment and its enjoyments, watches for futurity, and pants after remote felicity.

The general temperature of our climate, in the vicinity of the capital, is ſomewhat lower than the 50th degree of Fahrenheit's thermometer*; and a mean of the hotteſt or coldeſt months of our year rarely varies more than ten degrees from this ſtandard heat †. Winter therefore with us is uſually accompanied with a temperature of forty degrees, ſpring and autumn of fifty degrees, the ſummer of ſixty degrees of the thermometer: and the general heat of any ſingle month of theſe ſeveral ſeaſons ſeldom varies much from the correſponding temperature of that particular ſeaſon to which it belongs.

Of theſe limits the loweſt is not ſufficiently cold to check the growth of any of the natural herbage of our iſland, nor the higheſt powerful enough to parch the ſurface of a moiſt ſoil, or to ſcorch its luxuriant graſſes. Hence it comes to paſs that our fields maintain a perpetual verdure, unimpaired by either ſolſtice. Hence too the farmer is enabled to lay his lands under graſs almoſt at any ſeaſon, even at the commencement of winter ‡: and hence the grazier never loſes the benefit of his rich paſtures at any period of the year, unleſs during the tranſient paſſage of a temporary drift of ſnow; ſo that horſes, cattle, and ſheep, arrive here, with little care, at a degree of perfection unattainable in other countries without infinite expence and trouble.

For the growth of flax, the ſtaple commodity of the kingdom; of potatoes, the general food of its inhabitants; and of barley, from whence the ardent ſpirits and other fermented liquors of the country are derived; the climate ſeems to poſſeſs a peculiar felicity of tem-

* See Hamilton's paper on the temperature of Ireland, in the ſecond volume of theſe Tranſactions.

† See State of the Weather in Dublin, from June 1791 to June 1793, by Mr. Kirwan, Vol. V. of theſe Tranſactions.

‡ See Young's Tour in Ireland, Vol. I. p. 130.

perature,

perature, and moist in all its seasons; and for their necessary degree of ripeness the heat of summer is amply sufficient.

It is for the more delicate species of garden-fruits, for ill conducted crops of oats in bleak and mountainous situations, and for the complete and perfect maturity of wheat, that uneasy apprehensions can ever be entertained by the gardener or the farmer.

The former are in themselves of little importance; and, where such luxuries are required, glass coverings and artificial heat afford an ample supply, and remove all pretence for discontent. Human art and industry, attentive to circumstances, and accommodated to times, has stolen from the mild temperatures of our winters as much warmth as serves to compensate for the deficiency of summer heats in forwarding and perfecting the crops of wheat; and a little will teach the farmer in our mountainous provinces to use the same artifice with respect to his oats, whenever necessity shall demand his increased attention.

To sum up matters then with truth and brevity—A density of population, surpassing that of the vaunted millions of undepopulated France *; a copious export-trade in provisions of various kinds, unequalled by any kingdom whose inhabitants are proportionably numerous †; and a staple manufacture, unrivalled in general use, in certainty of produce, and intrinsic value ‡; are circumstances which have not fallen to the lot of other nations, and bring with them clear and irrefragable evidence to demonstrate a salubrious country, a genial climate, and a fertile soil in Ireland.

* The inhabitants of Ireland amount to more than four millions. (See Mr. Bushe's Memoir, Irish Trans. Vol. III.)

Hence the density of its population is at the rate of 182 persons to a square geographical mile, supposing the island to contain in round numbers 22,000 superficial miles.

Mr. Zimmerman, in his Political Survey of Europe, erroneously supposing the inhabitants of this country less than two millions and a half, deduces a density of population only to 117 persons on each mile. The following table marks the number of individuals on a geographical square mile in the northern countries of Europe, as taken from Zimmerman's tables (Ireland being corrected); from whence it will appear that this kingdom rates extremely in the density of its population.

	Persons.
Denmark - - -	12
Sweden - - -	14
Russia - - -	20
Scotland - - -	51
Poland - - -	53
Prussia - - -	67
England - - -	129
Germany - - -	135
France - - -	152
Ireland - - -	182
Holland - - -	236

† Two hundred and twenty thousand barrels of beef and pork, independent of the various matters connected therewith, and of every other species of provision, were exported in the year ending 25th March 1793.

‡ After a progressive increase of ninety years the exports of linen-cloth alone, in the year 1792, amounted to forty-three million yards, from one million only at the commencement of the present century.

Conclusion.

Conclusion.

IN this Memoir I have endeavoured to prove, from natural and almoft incontrovertible regifters of the phenomena of later years, that the winds, and particularly the weftern ftorms, have fwept with increafed violence over Ireland.

From this fact I have deduced a neceffary change in its climate; a more general equability of temperature through the year; fummers lefs warm, and winters more mild and open: and laftly, I have endeavoured to fupport this conclufion by general obfervation, and the enumeration of particular inftances where the defect of fummer heat, and winter ice, feem to be the moft ftrongly marked.

One queftion ftill remains curious in its principle, and interefting in its folution—Why have thefe weftern winds blown with unufual and increafed violence?

The limits of a Memoir fuch as this do not permit me to enlarge on this fubject, and even appear to include a reproof for the length of the prefent interruption. I fhall therefore trefpafs no longer on the moments of the Academy than to fuggeft a few queries, which may ftand over for future confideration; leaving it to time, and the ingenuity or better directed obfervations of others, to verify, to difprove, or to condemn, the hints which may be contained in them.

1ft. Have not our winds become more violent, and the temperature of our feafons more equable, fince the forefts of Ireland were cleared, and the country cultivated? And have not thefe winds, and that equability of temperature, been nearly proportioned to thefe as to their caufes?

2d. Have not fimilar changes occurred under analogous circumftances in North America; even in Canada, that country of extremes in heat and cold? And did not the ifland of Bermudas, though fituated fo much to the fouthward of us, become barren of fruit in confequence of the deftruction of its timber-trees?

3d. Has it not appeared from obfervations on the afcent of balloons, and the motion of clouds, that the lower mafs of air often purfues a different courfe from the upper ftratum? May not then the limits of our ftormy currents of air be often confined within a few hundred yards of the furface of the earth? And if fo, is it not poffible, and even probable, that the frequent interruption of forefts and groves, and hedge-row trees, might have formerly very much retarded, and finally checked, the progrefs of a tempeft?

4th. Have not all the countries of Europe, Afia, and America, within the parallel of Ireland, been very much denuded of their forefts within the prefent century? And has not the increafed velocity of the wefterly winds been proportioned to this deftruction of the forefts and trees, as to their natural caufes?

5th. Is it not probable, fince the prevalent winds of our parallel have a wefterly tendency, that circumftances which have removed impediments to their career round the entire globe would give increafed velocity to their courfe?

6th. Should not Ireland, launched as it is into the Atlantic beyond the other coafts of Europe, and denuded as it has been of forefts and hedge-row trees, be moft fenfibly affected by increafing tempefts from the weft, and the firft to experience their influence on its climate?

Whether thefe queries have any foundation in nature, or are merely to be efteemed the refult of an unreftrained imagination; whether fo diminutive an animal as man, fo temporary

porary in duration, ſo impotent in ſtrength, acting through the lengthened period and perſevering efforts of a large portion of his ſpecies, can reaſonably be deemed equal to the involuntary production of ſuch vaſt effects; to a change even of the elements and climates of the earth, may admit of doubt, of oppoſition, and even of denial. For theſe reaſons I have taken the liberty ſimply to propoſe them as matters of enquiry; and only beg the indulgence of the Academy for troubling them with matters that may not be attended with ſufficient evidence to bring conviction, or even to eſcape cenſure.

POSTSCRIPT.

October 20, 1795.

THE preſent year (1795) has been comparatively cold in its commencement, and warm in its ſummer: it has been calm, dry, and in many reſpects a contraſt to the long ſeries of preceding years, whoſe effects are regiſtered in this Memoir.

Yet the extremes of its temperature have not been equal to thoſe of former years, as far as can be inferred from the recorded effects of heat and cold on natural objects. The large rivers of the northern province have not been bound firmly in the icy chains of winter, nor have the grapes ripened in favourable aſpects in any part of the kingdom during ſummer, as formerly occurred in ſeaſons whoſe temperature was denominated extreme.

III.

Experiments and Obſervations on Electricity—Excitation—the two States—Points of Difference between the Action of weak and ſtrong Electricities compared together. (*W. N.*)

THE excitation of electrics by friction ſtill remains a myſtery. We poſſeſs nothing in the leaſt reſembling a theory of the changes of electrical capacity which the rubber and cylinder undergo during their change of relative poſition in contact. In the year 1789 I communicated ſeveral facts to the Royal Society* concerning the mutual action of the ſilk flap and the electrical cylinder upon each other, and ſhewed that ſomething like compenſation takes place while they remain together. The tranſition of the ſurface of the cylinder from the compenſated to the uncompenſated ſtate is ſtrikingly ſeen when a hole is cut in the ſilk, and the cylinder turned under circumſtances of conſiderable excitation. A caſcade of fire iſſues from that edge of the hole which is neareſt the cuſhion; but inſtead of being diſſipated into the air, it bends down again, and unites with the cylinder at the oppoſite edge of the hole, whence it proceeds as uſual to the receiving part of the prime conductor.

The phenomena of the two electricities called plus and minus are ſingularly diſtinct in almoſt every experiment which can be made with the exhibition of electric light. Paper is a good ſubſtance for obſerving the viſible paſſage of electricity. If a ſtrong plus electric ſtream be let fall on the flat ſide of an uninſulated ſheet of paper, it forms a beautiful ſtar about four inches in diameter, conſiſting of very diſtinct radii not ramified. The minus electricity, in circumſtances perfectly ſimilar, throws many pointed bruſhes to the paper,

* Philoſophical Tranſactions, No. lxxix.

but

but forms no ſtar upon it. In this experiment I uſed a machine with a cylinder of ſeven inches diameter.

Hence it ſeems to follow, that a hollow ball of paper, or a glaſs globe covered with paper, might form an amuſing part of the electrical apparatus for experiments in the dark.

The laws of action with regard to weak electricities and thoſe of conſiderable intenſity appear to differ in various particulars, which require further inveſtigation. When a number of jars are charged in ſucceſſion, that is to ſay, by inſulating the whole ſeries, and cauſing the outer coating of the firſt to communicate with the inner of the ſecond, and the outer coating of the ſecond with the inner of the third, and ſo forth, it is well known that the whole may be charged by communicating electricity to the inſide of the firſt, provided the laſt have a communication with the earth; but with this circumſtance, that the charge will reſtore itſelf by exploſion when the quantity of electricity is much leſs than the firſt jar would ſingly have received and retained. From this reſult it has with ſome probability been concluded, that glaſs reſiſts the communication of electric energy, and that the jars are ſucceſſively leſs and leſs charged. Whether this laſt concluſion be true, has not I believe been aſcertained by experiment.

Under all the uncertainties concerning the place occupied by the electric charge of coated glaſs, though it may ſeem unfair to make any inference reſpecting glaſs which is uncoated, yet, upon the whole, there appears to be a probability that the interpoſition of naked glaſs may impede the action of electrified bodies. This queſtion more immediately points at the tube in which the gold-leaf electrometer of Bennet is incloſed. To determine whether the tube of the electrometer does affect the electric ſtate of the included leaf, either by compenſation or otherwiſe, I took a piece of window-glaſs eighteen inches long, two inches wide, and one-twentieth of an inch thick, which I cleaned very well, and then paſſed it ſeveral times through the hot air over the flame of a candle. In this ſtate one end of the glaſs was laid gently upon the electrified plate of Bennet's electrometer, and then ſuddenly raiſed by a turn of the wriſt. It was ſcarcely poſſible to diſcern that the leaves were at all affected; but when the electrometer was in the plus ſtate a very ſlight collapſion was produced by raiſing the glaſs, and the contrary effect was produced when the electrometer was negative. Some days afterwards the experiment was repeated, after the gold-leaf had been changed for other pieces, which were very pointed and delicate in their movements. The reſult was, that the glaſs was always ſhewn by the electrometer to be in a weak poſitive ſtate; and, when the electricity of the electrometer was made plus, the collapſion was equal to the divergence when it was minus.

In making theſe experiments I had previouſly ſuppoſed that the influence of the metallic ſtate of the electrometer would produce ſomewhat of the nature of a charge upon the glaſs; and conſequently that the intenſity of the leaves would have been diminiſhed during the exiſtence of that charge; and alſo, that in ſuch a caſe the action of the metal through the glaſs would be ſubject to the ſame diminution as in the ſeries of jars. But as the glaſs did not appear to act in this manner, it ſeems proper to conclude that clean glaſs does not affect the electric ſtate of bodies by its vicinity, and that the divergence of the balls or the gold-leaf in the electrometers of Cavallo and Bennet is not diminiſhed by the tube which ſurrounds them.

From

From a variety of experiments it was clearly aſcertained that the metallic coatings, though by their vicinity they may diminiſh the intenſity of the electric ſtate in the leaves, do nevertheleſs increaſe the angle of divergence by their attraction.

When the gold-leaf electrometer is made with a very ſmall tube, its ſenſibility is ſomewhat increaſed by the nearneſs of the coatings; but the chance of rendering it unſerviceable from caſual friction, which excites the glaſs, and cauſes the gold leaf to ſtick to it, together with the leſs perfect view of the divergence through a tube of ſmall curvature, afford reaſons why a diameter of leſs than an inch ſhould be rejected. Other reaſons of convenience indicate that the diameter of the glaſs ſhould not much exceed this quantity.

I was once induced to think that the conſiderable magnitude of the cap of Bennet's electrometer might render it leſs capable of being acted upon by ſmall quantities of electricity. Experiment did not however give much countenance to this ſuppoſition. By trials with heads of different ſize, the ſmalleſt were found to be rather more ſenſible to extremely minute electricities, and leſs ſo to ſuch as were greater. The influence of very weak electricity may produce the oppoſite ſtate in the whole of a ſmall head, but only in part of a larger; the remaining part of this laſt aſſuming the oppoſite ſtate, and robbing the leaves of part of their intenſity. But in higher electricities the whole of the large head may be urged to give electricity to the leaves, in a quantity which the ſmaller head could not give without acquiring a higher degree of intenſity, and conſequently more ſtrongly reſiſting the deſired proceſs. It appears therefore that the maximum of effect with a given electricity, acting without communication, will not be obtained but by an head of a definite figure and magnitude.

From ſome experiments of Hoadley and Wilſon with a number of prime conductors ſeparately inſulated, it was eſtabliſhed that an electrified body brought near one end of ſuch a ſet of conductors, forming by contact a right line, will produce the contrary ſtate in the nearer conductors, and the ſame ſtate in thoſe which are more remote. Or, upon the hypotheſis of a ſingle fluid, the electricity of the body preſented will repel that of the compound conductor; ſo that by ſeparating them before the influence is removed, and afterwards examining them, the reſpective ſtates of the ſeveral portions of the whole line may be aſcertained.

It was a concluſion obviouſly enough deducible from this experiment, that if two electrometers of Bennet were connected by a metallic bar, there ought to be a difference when an electrified body is preſented at either extremity, or at the middle of the bar. I made this experiment with a braſs bar eighteen inches long. Both electrometers were affected at the ſame inſtant. Their divergence was equal, and the effect was the ſame, whether the glaſs or ſealing-wax was preſented at either end or at the middle of the bar. This reſult ſeems to indicate a difference in the mode of action between the very weak electricity I uſed and the much ſtronger of Hoadley and Wilſon.

The action of pointed bodies and of flame likewiſe exhibits a remarkable difference between the ſtrong and the weak electricities. The prime conductor of an electrical machine is very ſuddenly deprived of the greateſt part of its electricity by the operation of a metallic point; but does not ſeem to be much affected by the vicinity of burning candles;

candles. On the contrary, the electrometer of Bennet will ſcarcely communicate the leaſt portion of its electricity to a point not abſolutely in contact with its cap; but is readily deprived of its electric ſtate by a candle.

Coulomb, by his method of torſion, has determined that the action of weak electricities diminiſhes as the ſquare of the diſtance. I do not know that any experiments have been made to determine the ratio of the repulſion or attraction of bodies by electricity, with regard to the diſtance, when the intenſities are conſiderable.

To examine this object I cauſed a braſs conductor to be made, four inches in diameter, with a ſpherical part at each end, of five inches diameter; and, on the whole, twenty inches long. It was fixed with its axis in the vertical poſition. In the upper ſpherical part there was a ſtage ſupporting a pulley, the axis of which ran upon two pair of very delicate friction wheels. The lower ſpherical part was perforated in the axis, to receive the ſtem of a very light ſphere of paper gilt, and of the diameter of five inches. Care was taken that this ſtem, which was nearly the whole length of the conductor from the friction wheels to the lower orifice, ſhould not touch that orifice; and for greater ſecurity there was a ſmall frame carrying three friction rollers, againſt one of which it was ſure to bear in caſe of accidental irregularity. The upper part of the ſtem was faſtened to a fine ſilken thread which paſſed over the pulley, and was tied to a counterpoiſe adapted by its figure to receive ſmall weights, in order that the equilibrium or preponderance might be adjuſted at pleaſure. On the axis of the pulley was an index and hand, which moved upon a face without touching, beneath a convex glaſs, and ſhewed the deſcent of the gilded ball in inches and parts. I expected to have obtained a conſiderable ſcale on my ſtem of twelve inches. But in the trials I made when it was finiſhed, in which the aſcending power of the ball could be varied at pleaſure by addition to the counterpoiſe, I found either that it did not move at all, or that, if it did, it ran with conſiderable velocity through its whole length. This unexpected event, together with ſome other imperfections in the inſtrument itſelf, which required amendment and alteration, prevented me from purſuing and diverſifying a courſe of experiment that promiſed ſo little. The facts appear however to ſhew, that in great intenſities and ſhort diſtances, the diminution of effect, if it follow the law of the ſquares of the diſtances, is, like the attraction of the earth upon projectiles, too ſmall to be perceived. But from the indications afforded by Henley's quadrant electrometer, and the floating electrometer of Nollet, there appear to be reaſons why the purſuit ſhould not be thought unworthy of further experiment.

IV.

Analyſis of the Red Lead of Siberia; with Experiments on the New Metal it contains. By Citizen VAUQUELIN, *Inſpector of Mines, and Conſervator of Chemical Products at the Mineralogical School.*

(Concluded from page 393, Vol. II.)

Combinations of the Acid of Red Lead with the Alkalis.

THE acid of red lead forms, in its combination with the alkalis, ſalts which are ſoluble, cryſtalliſable, and coloured. The ſimpleſt proceſs for making theſe ſalts conſiſts

in

in boiling upon one part of red lead reduced into fine powder, two parts of carbonate of alkali with forty parts of water. By this means a double affinity is made to operate, by virtue of which carbonate of lead is formed, and falls to the bottom, while a combination of the acid of red lead with the alkali made ufe of remains fufpended in the water, by virtue of its folubility.

The proportion of alkaline carbonate proper to be ufed for the decompofition of the native red lead muft vary according to the proportions which fubfift between the bafes, the acids, and the water of cryftallization. The proportions ftated in the laft paragraph are fuited to the carbonate of pot-afh; but the proper quantities in the other cafes are eafily afcertained, by ufing a fmall quantity of the carbonate at firft, and afterwards gradually increafing it, till the greateft part of the red lead is decompofed. On the whole, it is better to ufe a fmaller than too large a portion of the carbonate, in order that its bafe may be more completely faturated with the acid of the red lead.

There is no reafon to fear a fimilar inconvenience with the carbonate of ammoniac. It is proper, on the contrary, to add more than is fufficient to faturate the acid of the red lead; becaufe a portion of this falt will always be driven off during the ebullition which is indifpenfably neceffary to produce the intended decompofition. And even in cafe there fhould remain an excefs after the complete decompofition of the red lead, there would be no danger, becaufe this excefs of alkali is driven off by the evaporation of the falt newly formed, and there is always a certainty of obtaining the combination perfectly pure.

The colour of the combinations of the acid of red lead with the alkalis is an orange yellow *. Their folutions afford cryftals nearly of the fame tinge, the differences being only flight variations of fhade. Thefe falts are decompofed by barytes, lime and ftrontian. The mineral acids likewife decompofe them by an inverfe operation.

Thefe falts afford oxygen gas by the action of heat, and their refidue has the appearance of a green mafs. The ammoniacal compound muft however be excepted, as its bafe is partly decompofed by the oxygen of the acid, and leaves in the retort a pure green oxide, the undecompofed portion of ammoniac being alfo driven off by the heat.

Thefe falts decompofe by double affinity the calcareous, barytic, magnefian, aluminous falts, &c. Moft of the metallic falts are likewife decompofed by thefe fubftances, and afford new combinations fcarcely or not at all foluble in water; and for the moft part red, yellow, orange, or lemon colour.

I have not hitherto been able to determine the figure of thefe falts with accuracy, nor their degree of folubility in water, for the reafons I have already explained; neither have I made all the combinations of this acid with the metallic oxides, nor determined its mode of action on the metals. Such experiments would no doubt have afforded interefting phenomena for the completion of the hiftory of this metallic acid.

But I indulge the hope that thefe experiments are only retarded, and that fome fortunate opportunity will hereafter enable us to perform them: and, at all events, the experiments already made will be fufficient to afcertain the prefence of the acid of red lead wherever it may be met with, and diftinguifh it from every other natural fubftance.

* That of the ammoniacal falt, which has the form of plated cryftals, is yellow, and exhibits the metallic brilliancy of gold. V.

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SECTION VII.

Reduction of the Acid of Red Lead to the Metallic State.

THOUGH the properties of the acid of red lead, defcribed in the foregoing paragraphs, are in ftrictnefs fufficient to convince thofe who are in the habit of treating metallic fubftances, that this fubftance belongs to the fame clafs, it is neverthelefs defirable, in order, as it were, to add the ftamp of evidence to the proofs of demonftration, that this acid fhould be reduced to the metallic ftate.

To obtain this object I took 72 parts of the acid in queftion, extracted from the red lead by the muriatic acid, as defcribed in our fourth fection, which I introduced into a crucible of charcoal placed in another of hard porcelain likewife filled with charcoal powder.

This apparatus, placed in a forge furnace, was expofed for an hour to the action of a very ftrong fire urged by the blaft of a ftrong pair of bellows with three pipes.

When the crucible was cooled and broken, I found, to my great fatisfaction, in the fmall veffel of charcoal, a metallic mafs of a white grey colour, fhining, very brittle, and upon the furface of which were many feathered cryftals of the fame colour perfectly metallic. This metallic mafs weighed forty-three parts.

It appears, from the refult of this operation, that the oxygen does not adhere with very great force to this metallic bafe, and that the acid contains about 0,40 of its weight of this acidifying principle.

SECTION VIII.

Properties of the Metal of Red Lead.

THIS metal, as I have already remarked, is of a white colour inclining to grey: it is very brittle, and cryftallifes in an elevated temperature into a kind of feathered filaments, which rife above the metallic mafs.

The metallic button being broken, prefented internally compact points forming a clofe grain, and in other parts needles croffing each other in all directions, leaving void fpaces between them, which prevented me from determining its fpecific gravity.

A fragment of this metal expofed to the heat of the blow-pipe acquired a tarnifh on its furface, and by continuing the operation it was covered with a light green cruft; but it afforded no fign of fufion.

When heated by the fame apparatus with borax it did not melt, but was in a fmall degree diminifhed, and communicated an emerald-green colour to the falt.

When reduced to a fine powder, and treated with the concentrated boiling nitric acid, it was oxided with much difficulty, and communicated to the acid a green tinge flightly inclining to blue. The nitric acid attacks this metal with fo much difficulty, that it was not till after treating it repeatedly with confiderable quantities of that folvent that I could fucceed in diffolving fix grains.

The different folutions being added together were evaporated to drynefs. Towards the end of the operation the refidue affumed the form and the ductility of a vegetable extract of a red brown colour. Cauftic potafh poured on this refidue diffolved a great part, and affumed a lemon-yellow colour; but part remained of a very beautiful green, which was not acted upon by that re-agent: it was a portion of the metal which had not received

from the nitric acid the whole quantity of oxygen neceſſary for its acidification. It was ſtill in the ſtate of oxide.

I treated it again with the concentrated nitric acid; and by a ſeries of operations of this nature I at length ſucceeded in completely acidifying it.

The combination of this artificial acid with potaſh exhibited abſolutely the ſame appearances with the different re-agents as the ſalt which is formed with the natural acid. See Section II.

SECTION IX.

Denomination of the Metal contained in the native Red Lead.

WHEN an unknown ſubſtance is to be examined, the only method to aſcertain whether it has been before deſcribed, is to examine its properties, and compare them with thoſe of other bodies; an operation which ſuppoſes a knowledge of all that has been before deſcribed in natural hiſtory. And when, after an accurate compariſon of the properties of the body under examination with thoſe of other bodies, it is found that none of theſe laſt exhibit the whole of thoſe properties, a fair concluſion may be formed that the body is unknown, and conſequently that it is new.

After this point is determined, it becomes neceſſary, in order to make it known to others, that its diſtinctive characters ſhould be clearly aſcertained, and a name given to it, for the purpoſe of brief deſignation, and of inſcription in the catalogue of human knowledge.

This name may be derived from various ſources;—the place whence it was obtained, the author who diſcovered it, or the ſpecific properties it poſſeſſes, &c.: but it is eaſy to ſee that the two firſt ſources are vicious. In fact, the name of the place ſeems to announce that the ſubſtance is to be found excluſively there; and it is very far from being known, at the early period of diſcovery, whether it may not be found elſewhere. The name of the author teaches nothing, except that ſuch a man was the diſcoverer; a circumſtance of little intereſt to ſcience. But the name of a ſubſtance deduced from its leading properties is truly uſeful, becauſe it brings to mind, and in ſome meaſure places the object before the eyes by a faithful ſketch of its attributes.

From theſe conſiderations I have thought fit to adopt the name *chrome*, which was propoſed to me by Cit. Haüy, to deſignate the new metal found in the native red lead. In truth, this name does not perfectly agree with the complete metal, becauſe it has no very diſtinct colour; and becauſe, even if it had one, this would not be a ſufficient reaſon, ſince every metal has a more or leſs peculiar colour.

But it agrees wonderfully well with its combinations with oxygen, which afford a green oxide, or a red acid, according to the proportions of that principle, and becauſe each of its primary combinations communicates its colour to all the ſecondary combinations into which it enters; properties which belong to it almoſt excluſively.

This name appears to me to be ſo much the better founded, as this ſubſtance has not yet been diſcovered except in the ſtate of acid or oxide, and becauſe it may perhaps never be found in the native metallic ſtate.

Neverthelеſs, as I am not diſpoſed to conſider the adoption of one name rather than another as of any importance, provided the name do expreſs ſome remarkable or diſtinctive property of that which it denotes, I ſhall with pleaſure, if a better name be found, conſent to ſubſtitute it inſtead of *chrome*.

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SECTION X.

Uses of Chrome, of the Oxide of Chrome, and of the Chromic Acid.

THE brittleness of chrome, its resistance to the action of fire, and the small masses in which it has hitherto been found, afford little expectation that this metal will ever be of great use in the arts. But this assertion may be too strong; for a new substance, of which the properties at first appear to be of small interest to society, is sometimes found, in the subsequent processes of investigation, to be capable of very important applications in the arts and sciences.

The acid and the oxide of this metal are certainly not subject to the same observation. The former, by the beautiful emerald-green which it communicates even to enamels, without being subject to alteration in the purity of its shade, will afford to painters and enamellers an additional object to enrich their productions, and add to the perfection of their art: the second, by the beautiful cinnabar red which it assumes and keeps in its combination with mercury; the orange-red colour it affords with lead; the carmelite red it communicates to silver, may become highly valuable for paintings in oil and water-colours.

Chemistry will be greatly benefited by this excellent re-agent to detect the smallest quantities of mercury, silver, and lead, dissolved in acids by the different colours it produces when its action is assisted by an alkali. And, in the same manner as this acid indicates the presence of the metals here spoken of, those metals in their turn may serve to discover the chromic acid, if it be previously put into the necessary condition to produce the before-mentioned effects.

If the chromic acid should hereafter be abundantly found in any other combination besides that of lead, it might, after extraction by carbonate of potash, be used for the artificial composition of red lead, and furnish, in abundance, this valuable orange-red colour for painters' use, which is sold very dear in Siberia, and applied to this purpose with great success. For this object the native red lead, which is disseminated in small crystals, or plates, in the fissures of quartzose or gritstone gangues, in which it is commonly found, might be pulverized, then boiled with a solution of carbonate of potash, and mixed with nitrate of lead; the acid of which will saturate the potash, and afford its metal to the new acid, which will thus produce a colour no less beautiful than the natural, and perfectly clear of the matrix.

There is reason to presume that chrome, either in the state of oxide or of acid, will be found singly or engaged in some other combinations. For the analysis of the emerald of Peru has already shewn me that its colouring part is afforded by the oxide of this metal; a circumstance which gives the most agreeable proof of the goodness and fixity of this colour, as it is known that the emerald can sustain the most violent degree of heat without losing its colour.

I have likewise found that the yellowish green tufted crystals, which often accompany the red lead ore of Siberia, are formed of chrome and lead, both united, in the state of oxide. Green crystals possessing the same form, the same dimensions, and the same situations on the matrix as those of red lead, but which are a combination of the oxides of chrome and of lead, are likewise found in certain specimens of the native red lead. It is probable that these combinations originally existed in the state of chromate of lead, and that in process of

of time a portion of oxygen having been carried off by unknown causes, converted them to the state of oxide, and changed the red colour to green.

From paragraph III. it follows that the Siberian red lead contains

			By analysis. Experiment V.	By synthesis. Experiment VI.
Oxide of lead	-	-	63,96	65,12
Chromic acid	-	-	36,40	34,88

V.

Experiments to determine the Density of the Earth. By HENRY CAVENDISH, *Esq. F. R. S. and A. S.**

MANY years ago the Rev. John Michell, F. R. S. contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian professor at Cambridge, who, not having conveniences for making experiments with it in the manner he could wish, presented it to Mr. Cavendish.

The apparatus is very simple: it consists of a wooden arm six feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position by a slender wire forty inches long, and to each extremity is hung a leaden ball about two inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre than is necessary to twist the suspending wire, it is plain that, if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. Michell intended to use were eight inches diameter. One of these was to be placed on one side of the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and consequently half the difference of these positions would shew how much the arm was drawn aside by the attraction of the weights.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. Michell intended to do by putting the arm in motion, and observing the time of its vibrations, from which it may be easily computed†.

* Abridgment of his paper in the Philosophical Transactions, 1798, p. 469.

† Mr. Coulomb has in a variety of cases used a contrivance of this kind for trying small attractions; but Mr. Michell informed Mr. C. of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.

Mr. Michell had prepared two wooden ſtands, on which the leaden weights were to be ſupported and puſhed forwards till they came almoſt in contact with the caſe; but he ſeems to have intended to move them by hand.

As the force with which the balls are attracted by theſe weights is exceſſively minute, not more than one fifty-millionth part of their weight, it is plain that a very minute diſturbing force will be ſufficient to deſtroy the ſucceſs of the experiment: and from the following experiments it will appear, that the diſturbing force moſt difficult to guard againſt is that ariſing from the variations of heat and cold; for, if one ſide of the caſe is warmer than the other, the air in contact with it will be rarefied, and in conſequence will aſcend, while that on the other ſide will deſcend, and produce a current which will draw the arm ſenſibly aſide *.

As Mr. Cavendiſh was convinced of the neceſſity of guarding againſt this ſource of error, he reſolved to place the apparatus in a room which ſhould remain conſtantly ſhut, and to obſerve the motion of the arm from without by means of a teleſcope, and to ſuſpend the leaden weights in ſuch a manner that he could move them without entering into the room. This difference in the manner of obſerving rendered it neceſſary to make ſome alteration in Mr. Michell's apparatus: and as there were ſome parts of it which Mr. Cavendiſh thought not ſo convenient as could be wiſhed, he choſe to make the greateſt part of it afreſh.

Plate XIX. is a longitudinal vertical ſection through the inſtrument, and the building in which it is placed. A B C D D C B A E F F E is the caſe, x and x are two balls which are ſuſpended by the wires $h\,x$ from the arm $g\,h\,m\,h$, which is itſelf ſuſpended by the ſlender wire $g\,l$. This arm conſiſts of a ſlender deal rod $h\,m\,h$, ſtrengthened by a ſilver wire $h\,g\,h$; by which means it is made ſtrong enough to ſupport the balls, though very light †.

The caſe is ſupported and ſet horizontal by four ſcrews, reſting on poſts fixed firmly into the ground: two of them are repreſented in the figure by S and S; the two others are not repreſented, to avoid confuſion. G G and G G are the end walls of the building. W and W are the leaden weights which are ſuſpended by the copper rods Rr PrR and the wooden bar $r\,r$ from the centre pin Pp. This pin paſſes through a hole in the beam H H, perpendicularly over the centre of the inſtrument, and turns round in it, being prevented from falling by the plate p. M M is a pulley faſtened to this pin, and Mm a cord wound round the pulley and paſſing through the end wall, by which the obſerver may turn it round, and thereby move the weights from one ſituation to the other. When the weights are in the poſition repreſented in the engraving, both conſpire, by their action on the ſmall weights $x\,x$, to draw the arm in the ſame direction; but when, by means of the pulley

* Mr. Caſſini, in obſerving the variation-compaſs placed by him in the Obſervatory (which was conſtructed ſo as to make very minute changes of poſition viſible, and in which the needle was ſuſpended by a ſilk thread), found that ſtanding near the box, in order to obſerve, drew the needle ſenſibly aſide; which I have no doubt was cauſed by this current of air. It muſt be obſerved that his compaſs-box was of metal, which tranſmits heat faſter than wood, and alſo was many inches deep; both which cauſes ſerved to increaſe the current of air. To diminiſh the effect of this current, it is by all means adviſable to make the box in which the needle plays not much deeper than is neceſſary to prevent the needle from ſtriking againſt the top and bottom. C.

† Mr. Michell's rod was entirely of wood, and was much ſtronger and ſtiffer than this, though not much heavier; but as it had warped when it came to Mr. Cavendiſh, he choſe to make another, and preferred this form, partly as being eaſier to conſtruct and meeting with leſs reſiſtance from the air, and partly becauſe, from its being of a leſs complicated form, he could more eaſily compute how much it was attracted by the weights.

MM, the observer causes them to move through a considerable portion of a semicircle, they will arrive at a second position, in which their joint action will move the arm in the contrary direction. These weights are prevented from striking the instrument by pieces of wood fastened to the wall of the building.

The situation of the arm is determined by slips of ivory placed within the case near each extremity of the arm. These slips bear a set of divisions of the inch into twenty parts; to which a small slip at each end of the arm applies, without contact, a vernier that affords a subdivision into hundredth parts. Estimation by the eye looking through the short telescopes T and T, gives the position of the arm to still greater nicety. The lamps L and L throw light on the divisions by convex glasses, and no other light is admitted into the room.

F K is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *g l* is fastened, and thereby enables the observer to turn round the wire till the arm settles in the middle of the case without danger of touching either side. The wire *g l* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws. The different parts are drawn nearly in the proper proportion to each other.

Suppose the arm to be at rest and its position to be observed, let the weights be then moved; the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion were destroyed, or the point of rest. To do this, Mr. Cavendish observes three successive extreme points of vibration, and takes the mean between the first and third of these points as the extreme point of vibration in one direction, and then assumes the mean between this and the second extreme as the point of rest; for, as the vibrations are continually diminishing, it is evident that the mean between two extreme points will not give the true point of rest.

It may, he adds, be thought more exact to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that, notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason it is best to determine the point of rest from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which he finds in this manner: He observes the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as he can guess, that these divisions shall be on different sides of the middle point, and not very far from it. He then completes the middle point of the vibration, and by proportion finds the time at which the arm comes to this middle point. He then, after a number of vibrations, repeats this operation, and divides the interval of time between the coming of the arm to these two middle points by the number of vibrations, which gives the time of one vibration. The propriety of this method is more fully explained in the Memoir itself.

In Mr. Cavendish's first experiments the wire by which the arm was suspended was $39\frac{1}{4}$ inches long. It was of copper silvered, one foot of which weighed $2\frac{4}{10}$ths grains; and its stiffness was such as to make the arm perform one vibration in about 15 minutes. It was immediately

immediately found that it was not ftiff enough, as the attraction of the weights drew the balls fo much afide as to make them touch the fides of the cafe. It was thought proper, however, to make fome experiments before a ftiffer wire was fubftituted in its place.

In this trial the rods by which the leaden weights were fufpended were of iron, the probability of magnetifm in which feemed to be of no confequence, as the arm contained nothing magnetical. Some trials however, for greater fecurity, were made with the rods alone. The effect was fuch as might juftify a very flight fufpicion of magnetifm; for which reafon the rods were changed for others of copper. When the weights were hung on, the attraction on the ball was very fenfible, as it carried the latter through fifteen divifions, or $1\frac{1}{2}$ inch; and the regularity of the refults was fuch, that the extremes did not differ from each other more than one-tenth part. It was found that the effect of the attraction feemed to increafe for half an hour or an hour after the motion of the weights; an effect which might be fuppofed to arife from a want of elafticity, either in the fufpended wire or fomething it was faftened to, which might make it yield more to a given preffure, after a long continuance of that preffure, than it did at firft. This fufpicion was put to the trial by keeping the wire for two or three hours in a ftate of torfion amounting to fifteen divifions, and then returning it by means of the handle K to its original fituation. It was not found by two repetitions of the experiment that the wire had acquired any fet or permanent twift. The arm was then fufpended by a ftiffer wire, with which the apparatus, inftead of performing its vibration in about fifteen minutes, employed only feven. The motion of the arm was in this cafe not quite fix divifions between the two oppofite pofitions of the weights, or the deviation from the natural ftation of the arm was half that quantity.

A variation of the power of the weights upon the balls, in fome refpect fimilar to that obferved with the fmall wire, was alfo found to take place in the prefent experiments. It happened that the cafe in which the arm was enclofed was placed nearly parallel to the magnetic eaft and weft; and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth—and the weights alfo, after remaining for fome time in either of the near pofitions, would acquire a polarity of the fame kind, and attract the balls more ftrongly on that account; and on the contrary, when removed to the oppofite near pofition, if the magnetifm continued permanent in the weights for fufficient time, the magnetic power would operate in the way of repulfion. This was in fact the kind of effect pointed out by the obfervations. The accurate habits which fo eminently diftinguifh all the experimental proceffes of Mr. Cavendifh did not however permit him to reft contented with the induction to which thefe facts feemed to point. He altered his apparatus fo as to produce at pleafure a rotation in the weights upon their vertical axes, without opening the room or difturbing their actual fituation. By this contrivance he had it in his power to difpofe the imaginary magnetic poles in fuch pofitions as were beft calculated to detect their operation. The arm was not affected; and confequently it follows that the irregularity in queftion did not arife from magnetifm. This conclufion was ftill more firmly eftablifhed by fubftituting two actual magnetic bars in the place of the weights; for it was not obferved that any alteration was produced by reverfing the pofition of their poles.

The next circumftance that fuggefted itfelf was, that the effect might be owing to a difference of temperature between the weights and the cafe; for it is evident, that if the weights

weights were much warmer than the caſe, they would warm that ſide which was next to them, and produce an aſcending current of air, which would be accompanied by a deſcending current towards the oppoſite ſide, and two horizontal currents near the top and bottom; the latter of which would impel the ball towards the weight. Though it ſeemed ſcarcely probable that the weights ſhould happen to be warmer than the caſe, and that in a ſufficient degree to afford any perceptible conſequence, our author nevertheleſs determined to ſubmit the matter to examination. He therefore placed two lamps under the weights while in the mid-way poſition, and placed a thermometer with its ball cloſe to the outſide of the caſe, near that part which one of the weights would approach when brought into the poſition to exert its attraction upon the ball. The door of the room was then ſhut, and ſome time afterwards the weights were moved to this laſt-mentioned poſition. At firſt the arm was drawn aſide only in its uſual manner; but in half an hour the effect was ſo much increaſed that the arm was drawn fourteen diviſions aſide inſtead of about three, as it would otherwiſe have been; and the thermometer was raiſed near $1\frac{1}{2}°$, namely from 61° to $62\frac{1}{2}°$. On opening the door, the weights were found to be no more heated than juſt to prevent their feeling cool to the hand.

From the great effect of a difference of temperature it became deſirable to aſcertain the temperature of the weight itſelf, and of the air near the caſe, in the ſubſequent experiments. A ſmall thermometer was therefore inſerted in one of the balls, and another placed cloſe to the caſe. A number of experiments were then made, in which the differences of temperature between the weights and the external air were obſerved; the weights being in ſome inſtances heated by a lamp, and in others cooled by means of ice. It was found that a higher temperature in the weight occaſioned a greater deviation or apparent attraction of the ball; and, on the contrary, that a lower temperature cauſed the ball to be driven by a current in the oppoſite direction.

After this minute and ſcientific inveſtigation of the cauſes which are capable of producing error in the reſults, Mr. Cavendiſh proceeded to make his fundamental experiments. Theſe are tabulated at length in the memoir itſelf, and followed by a detail of the method of computing the denſity of the earth. The firſt thing to be done is to determine from the time of a vibration, what force is required to draw the arm aſide; and the next, to find the proportion which the attraction of the weight upon the ball bears to that of the earth upon the ſame ball, ſuppoſing the ball to be placed in the middle of the caſe: and from theſe reſults (ſince the diameter and ſpecific gravity of the weights and balls compared with water, and the diameter of the earth, are alſo known; together with the facts that the quantities of matter are as the attractive forces at like diſtances, and vary in the inverſe duplicate ratio of the diſtance) the mean denſity of the earth itſelf may alſo be found. Several corrections requiſite to be made in this denſity are then ſtated; namely, on account of the reſiſtance of the arm to motion, the attraction of the weights on the arm, and on the remote ball; the attraction of the copper rods, and of the caſe itſelf; and for the alteration of the attraction of the weights upon the balls according to the poſition of the arm; which laſt is of more ſignification than all the reſt. For all which I muſt, for the ſake of brevity, refer to the Tranſactions.

The

The following TABLE contains the Results of the Experiments.

Experiment	Mot. weight.	Mot. arm.	Do. corr.	Time vibr.	Do. corr.	Density.
1.	m to +	14,32	13,42	′ ″	—	5,5
	+ to m	14,1	13,17	14,55	—	5,61
2.	m to +	15,87	14,69	—	—	4,88
	+ to m	15,45	14,14	14,42	—	5,07
3.	+ to m	15,22	13,56	14,39	—	5,26
	m to +	14,5	13,28	14,54	—	5,55
4.	m to +	3,1	2,95	—	6,54	5,36
	+ to —	6,18	—	7,1	—	5,29
	— to +	5,92	—	7,3	—	5,58
5.	+ to —	5,9	—	7,5	—	5,65
	— to +	5,98	—	7,5	—	5,57
6.	m to —	3,03	2,9			5,53
	— to +	5,9	5,71			5,62
7.	m to —	3,15	3,03	7,4 by mean.	6,57	5,29
	— to +	6,1	5,9			5,44
8.	m to —	3,13	3,00			5,34
	— to +	5,72	5,54			5,79
9.	+ to —	6,32	—	6,58	—	5,1
10.	+ to —	6,15	—	6,59	—	5,27
11.	+ to —	6,07	—	7,1	—	5,39
12.	— to +	6,09	—	7,3	—	5,42
13.	— to +	6,12	—	7,6	—	5,47
	+ to —	5,97	—	7,7	—	5,63
14.	— to +	6,27	—	7,6	—	5,34
	+ to —	6,13	—	7,6	—	5,46
15.	— to +	6,34	—	7,7	—	5,3
16.	— to +	6,1	—	7,16	—	5,75
17.	— to +	5,78	—	7,2	—	5,68
	+ to —	5,64	—	7,3	—	5,85

In the second column the letter m denotes the middle position of the weights when each is equidistant from the balls, + denotes one of the near positions, and — the opposite near position.

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I copy Mr. Cavendiſh's concluding remarks in his own words:

"From this table it appears, that though the experiments agree pretty well together, yet the difference between them both in the quantity of motion of the arm and in the time of vibration is greater than can proceed merely from the error of obſervation. As to the difference in the motion of the arm, it may very well be accounted for from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration is doubtful. If the current of air was regular, and of the ſame ſwiftneſs in all parts of the vibration of the ball, I think it could not; but as there will moſt likely be much irregularity in the current, it may very likely be ſufficient to account for the difference.

"By a mean of the experiments made with the wire firſt uſed, the denſity of the earth comes out 5,48 times greater than that of water; and by a mean of thoſe made with the latter wire it comes out the ſame; and the extreme difference of the reſults of the twenty-three obſervations made with this wire is only ,75; ſo that the extreme reſults do not differ from the mean by more than ,38, or $\frac{1}{14}$th of the whole, and therefore the denſity ſhould ſeem to be determined hereby to great exactneſs. It indeed may be objected, that as the reſult appears to be influenced by the current of air or ſome other cauſe, the laws of which we are not well acquainted with, this cauſe may perhaps act always or commonly in the ſame direction, and thereby make a conſiderable error in the reſult. But yet, as the experiments were tried in various weathers and with conſiderable variety in the difference of temperature of the weights and air, and with the arm reſting at different diſtances from the ſides of the caſe, it ſeems very unlikely that this cauſe ſhould act ſo uniformly in the ſame way as to make the error of the mean reſult nearly equal to the difference between this and the extreme; and therefore it ſeems very unlikely that the denſity of the earth ſhould differ from 5,48 by ſo much as $\frac{1}{14}$th of the whole.

"Another objection perhaps may be made to theſe experiments, namely, that it is uncertain whether in theſe ſmall diſtances the force of gravity follows exactly the ſame law as in greater diſtances. There is no reaſon however to think that any irregularity of this kind takes place until the bodies come within the action of what is called the attraction of coheſion, and which ſeems to extend only to very minute diſtances. With a view to ſee whether the reſult could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to reſt as cloſe to the ſides of the caſe as they could; but there is no difference to be depended on between the reſults under that circumſtance and when the balls are placed in any other part of the caſe.

"According to the experiments made by Dr. Maſkelyne on the attraction of the hill Schehallien, the denſity of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I ſhould have expected. But I forbear entering into any conſideration of which determination is moſt to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whoſe quantity I cannot meaſure."

VI. *An*

VI.

An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, *Count of Rumford, F. R. S. M. R. I. A.*

(Concluded from page 405.)

HAVING been fo fuccefsful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moiſt way*, I loſt no time in making ſimilar experiments with the oxide of ſilver.

Experiment No. 11. A ſolution of fine ſilver, in ſtrong nitrous acid, was evaporated by dryneſs, and the reſiduum re-diſſolved in diſtilled water.

A portion of this ſolution (which was perfectly colourleſs), diluted with twice as much diſtilled water, was poured into a phial containing a number of ſmall pieces of charcoal; and the phial, being well cloſed with a new cork ſtopple, was expoſed to the action of the ſun's rays.

In leſs than an hour ſmall ſpecks of revived ſilver began to make their appearance on the ſurface of the charcoal; and, at the end of two hours, theſe ſpecks became very numerous, and had increaſed ſo much in ſize, that they were diſtinctly viſible to the naked eye at the diſtance of more than three feet. They were very white, and poſſeſſed the metallic ſplendour of ſilver in ſo high a degree, that when enlightened by the ſun's beams their luſtre was nearly equal to that of very ſmall diamonds.

The phial, which was in the form of a pear, and about 1½ inch in diameter at its bulb, was very thin, and made of very fine colourleſs glaſs; the aqueous ſolution was alſo perfectly tranſparent and colourleſs; and, when the contents of the phial were illuminated by the direct rays of a bright ſun, the contraſt of the white colour of theſe little metallic ſpangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and intereſting ſight.

As the air had been previouſly expelled from the charcoal by boiling it in diſtilled water, it was ſpecifically heavier than the aqueous ſolution of the metallic oxide, and conſequently remained at the bottom of the bottle.

Experiment No. 12. A phial as nearly as poſſible like that uſed in the laſt experiment, and containing the ſame quantity of diluted aqueous ſolution of nitrate of ſilver, and alſo of charcoal, was incloſed in a cylindrical tin box, and expoſed one hour to the heat of boiling water in an apparatus uſed for boiling potatoes in ſteam for the table.

The reſult of this experiment was uncommonly ſtriking: the ſurface of the charcoal was covered with a moſt beautiful metallic vegetation; ſmall filaments of revived ſilver, reſembling fine flatted ſilver wire, puſhing out from its ſurface in all directions!

Some of theſe metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were eaſily detached from the ſurface of the charcoal, to which they ſeemed to adhere but very ſlightly.

Theſe experiments were repeated ſeveral times, and always with preciſely the ſame reſults.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of ſmall ſcales, adhering firmly to the ſurface of the charcoal. May not the difference of

of the forms under which gold and ſilver are revived from their oxides, in this proceſs, be owing to the difference of the ſpecific gravities of thoſe metals?

The following experiments, which were firſt ſuggeſted by an accident, were made with a view to inveſtigate ſtill farther the cauſes of thoſe effects which have been attributed to the ſuppoſed chemical properties of light.

Having accidentally put away two ſmall phials, each containing a quantity of aqueous ſolution of the oxide of gold and ſulphuric ether, in each of which the ether had extracted the gold completely from the ſolution, as was evident by the yellow colour of the ſolution having been transferred to the ether, and the ſolution being left colourleſs; in one of the phials, which happened to ſtand in a window in which there was occaſionally a ſtrong light (though the direct rays of the ſun never fell on it), I found, in about three weeks, that the oxide was almoſt entirely reduced; the revived gold appearing in all its metallic ſplendour in the form of a thin pellicle, ſwimming on the ſurface of the aqueous liquor in the phial, and the colour of the ether which repoſed on it having become quite faint; while no viſible change had been produced in the contents of the other phial, which had ſtood in a dark corner of the room.

As theſe appearances induced me to ſuſpect, or rather ſtrengthened the ſuſpicions I had before conceived, that the ſeparation of gold from ether, under its metallic form, when a ſolution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther inveſtigation of that matter.

Experiment No. 13. Into a ſmall pear-like phial of very fine tranſparent glaſs I put equal quantities of an aqueous ſolution of the muriatic oxide of gold and ſulphuric ether; and the phial, which was about half filled, being cloſed with a good cork well ſecured in its place, was expoſed to the action of the direct rays of a bright ſun.

A pellicle of revived gold, in all its metallic ſplendour, began almoſt immediately to be formed on the ſurface of the aqueous liquid, and ſoon covered it entirely; and at the end of two hours the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourleſs.*

On ſhaking the phial, the metallic pellicle, which covered the ſurface of the aqueous liquid, was broken into ſmall pieces, which had exactly the appearance of leaf-gold, poſſeſſing the true colour and all the metallic brilliancy of that metal.

On ſuffering the phial to ſtand quiet, the aqueous liquor and the ether ſeparated, and moſt of the broken pieces of the thin ſheet of gold deſcended to the bottom of the phial: the remainder of them floated on the ſurface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly tranſparent and *colourleſs.*

By the length of time which was required for the ether and the aqueous liquid to ſeparate, I thought I could perceive that the ether had loſt ſomething of its fluidity; but as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I ſaw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been ſuffered to cool, there was no appearance of any conſiderable quantity of air, or other permanently elaſtic fluid, having been either generated or abſorbed during the experiment.

Finding that the oxide of gold might be ſo completely and ſo expeditiouſly reduced by means

means of ether, I conceived it might be poſſible to perform that chemical proceſs, *in the moiſt way*, by means of eſſential oils; and this conjecture proved to be well founded.

Experiment No. 14. Upon a quantity of a diluted aqueous ſolution of nitro-muriate of gold, in a ſmall pear-like phial, about $1\frac{1}{2}$ inch in diameter at its bulb, was poured a ſmall quantity of etherial oil of turpentine, juſt as much as was ſufficient to cover the aqueous ſolution to the height of $\frac{2}{10}$ of an inch; and the phial, being well cloſed with a good cork, well ſecured, was expoſed one hour to the heat of boiling water in a ſteam-veſſel.

The gold was revived, appearing in the form of a ſplendid pellicle, of a bright gold colour, which floated on the ſurface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourleſs as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it repoſed, had entirely loſt its yellow colour.

On ſhaking the phial, its contents were intimately mixed; but, on ſuffering it to ſtand quiet, the oil of turpentine ſoon ſeparated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourleſs.

On ſhaking the phial, *before it had been expoſed to the heat*, and mixing its contents, and then ſuffering it to ſtand quiet, the oil of turpentine, on taking its place at the top of the aqueous ſolution, was not found to have acquired any colour; nor was the bright gold colour of the ſolution found to be at all impaired. When ſulphuric ether was uſed inſtead of the oil of turpentine, the effect was in this reſpect very different.

To find out whether the oil of turpentine uſed in this experiment, and which had acquired a deep yellow colour, had loſt that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous ſolution of the oxide into the phial, and, ſhaking the phial, expoſed it with its contents to the heat of boiling water.

After it had been expoſed to this heat about two hours I examined it, and found, that though a conſiderable quantity of gold had been revived, yet the aqueous liquid ſtill retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial I now added about half as much diſtilled water, and, mixing the whole by ſhaking, I expoſed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourleſs*.

On repeating this experiment with oil of turpentine, and varying it, by uſing a ſolution of the oxide of *ſilver* (an aqueous ſolution of nitrate of ſilver,) inſtead of that of *gold*, the reſult was nearly the ſame: the metal was revived, and the oil of turpentine acquired a faint greeniſh-yellow colour.

I alſo revived the oxides of gold and of ſilver with *oil of olives*, by a ſimilar proceſs, with the heat of boiling water. The oil of olives uſed in theſe experiments loſt its tranſparency, and became deeply coloured: that uſed in the reduction of the oxide of ſilver taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold being changed to a yellowiſh-brown, with a purple hue.

 In

In the experiment with the oxide of ſilver, the inſide of the phial, in the region where the oil repoſed on the aqueous ſolution, was beautifully ſilvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, ſmall detached pellicles of revived metal were viſible in the oil, and adhered in ſeveral places to the inſide of the phial, forming bright ſpots, in which the colour of the metal and its peculiar ſplendour were perfectly conſpicuous.

Experiment No. 15. As *carbon* is one of the conſtituent principles of ſpirit of wine, as well as of eſſential oils and ſulphuric ether, I thought it poſſible that I might ſucceed in the reduction of the oxide of gold, by mixing alcohol with an aqueous ſolution of nitro-muriate of gold, and expoſing the mixture in a phial well cloſed to the heat of boiling water; but the experiment did not ſucceed.

By pouring upon this mixture a ſmall quantity of oil of olives, and expoſing it again to the heat of boiling water, the gold was revived.

Is it not probable that the reaſon why the oxide was not reduced by alcohol, is the mobility of thoſe elements, which ought to act on each other, in order that the effect in queſtion may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to reſt on the ſurface of the aqueous ſolution without mixing with it.

I wiſhed to have been able to have collected and examined the elaſtic fluids, which probably were formed in moſt of the preceding experiments; but my time was ſo much taken up with other matters that I had not leiſure to purſue theſe inveſtigations farther.

In order to ſee what effects would be produced by the heat generated at the ſurface of an opaque body, of a nature different from thoſe hitherto uſed in the reduction of the metallic oxides, and one that is little diſpoſed to form a chemical union with oxygen, (*magneſia alba*) when, being immerſed in an aqueous ſolution of the oxide of gold, the rays of the ſun were made to impinge on it, I contrived the following experiment.

Experiment No. 16. I took four ſmall thin phials, A, B, C, and D, of very fine glaſs, and putting into each of them about five grains of dry *magneſia alba*, I filled the phial A, nearly full, with a ſaturated aqueous ſolution of the oxide of gold.

I filled the phial B, in like manner, with ſome of the ſame ſolution, diluted with an equal quantity of diſtilled water; and the phials C and D were filled with the ſolution ſtill farther diluted.

Theſe phials, open or without ſtoppers, were expoſed one whole day to the action of the direct rays of a bright ſun, their contents being often well mixed together during that time by ſhaking.

The contents of all theſe phials changed colour more or leſs, but they acquired very different hues. The contents of the phial A became of a very deep rich *gold colour*, approaching to *orange*, the earthy ſediment being throughout of the ſame tint.

The contents of the phial B, which were at firſt of a light ſtraw colour, firſt changed to a light green, and then to a greeniſh blue. The phial having been ſuffered to ſtand quiet ſeveral days, in an uninhabited room in a retired part of the houſe, the ſolution became nearly colourleſs, and the ſediment was found to be of a dirty olive colour.

The

The colour of the contents of the phials C and D was changed nearly in the same manner; and having been suffered to stand quiet two or three days to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.

VII.

Some Account of the Persian Cotton-Tree. By MATTHEW GUTHRIE, *M.D. F.R.S. &c. &c.**

COTTON is a plant of both the old and the new world; at least it is found wild in both: but I have my doubts whether it was a native of America before the Europeans carried it over, and shall assign reasons for my incredulity when I come to treat of the *Persian cotton*, which is the very species that is said to be American.

Five species of the cotton-tree are enumerated by Linnæus; and there is reason to suspect the existence of a sixth, if what we are told of the extreme fineness and silky nature of a particular kind reared in some of the Antilles be literally true. This curious variety is called Siam cotton, because the reed was originally obtained from Siam.

The first species of cotton is the *gossypium arboreum*, or *Indian cotton-tree*, which has been cultivated and manufactured in the East Indies from the remotest period of the authentic history of that country, or between three and four thousand years. It delights in a sandy soil.

The second species is the *gossypium religiosum*, which is likewise a native of India, and a tree, or at least a high shrub; but why Linnæus dignified it with so singular a specific name I shall leave the learned Asiatic Society in Bengal to determine, as they must know if it be used for any religious purposes by the Bramins. This species of cotton is said to be that which is cultivated by the French in Martinico.

The third is the *G. barbadense*, a species of biennial cotton shrub cultivated in our British island of Barbadoes, from which it obtains its specific name. I believe it is likewise the same species which is cultivated in Jamaica.

The fourth is the *G. hirsutum*, an American *perennial* cotton shrub in the warmer provinces, but *annual* in the colder, as is sometimes the case with plants in climates where their roots lose their vegetating power by winter frost.

The fifth and last species is the *G. herbaceum*, or *G. annuum*, an annual cotton plant which rises to the height of three or four feet, and is sown and reaped, like corn, twice a year

* Manchester Memoirs, Vol. V. Part I.

year in hot countries, and once a year in colder climates. It bears a large yellow flower with a purple centre, and fruit about the size of a walnut containing the cotton.

This is the famous *Persian cotton* properly the subject of the paper, although a slight mention of other species was necessary to give a more complete view of the subject. Linnæus calls it a native of America; and there is no doubt but that it is become so, although there is much more reason to suppose America naturalized a Persian plant than that Persia got it from the new world; especially if we are to credit a paper lately presented by a British merchant to the Economical Society at Petersburgh, in which it is positively asserted that several of the European nations furnished their American colonies with *Persian cotton* seed procured at Smyrna. Now this fact (if sufficiently authentic, which I do not doubt from my knowledge of the veracity of the author) will easily account for the *G. herbaceum* being found wild in America; when we recollect the wonderful provision of nature for the wide dispersion of seeds, and Linnæus's assertion that the *Erigeron canadense* was dispersed from the botanic garden of Paris by the winds over a great part of Europe, and several other plants* from the botanic garden of Upsal over a whole province.

My reasons for suggesting these doubts relative to the native country of this species of cotton are, that all vegetables of this genus are supposed to have been indigenous in Persia exclusively, and that even the East Indies derived the cotton plants from thence; a conjecture which seems to have acquired some degree of credit from the late discovery of Sir William Jones, viz. that the Hindoos, or inhabitants of India, were originally a colony of the ancient Iran or Persia, which seems to have been the cradle of the human species, since its ancient language appears to have been the mother of all those now existing (with the exception of the Arabic and Tartarian), of which nevertheless it contained many words.

Now it is very possible that the first colony carried the cotton plant with them to India, and that it was afterwards dispersed from Hindostan to the adjacent countries and islands. The cotton plant is widely dispersed likewise throughout Europe and some parts of Africa, particularly the annual or herbaceous species (the very plant treated of here) reared in the north of Persia, and which is also cultivated in Malta †, Sicily, Chio, Lemnos, and other islands of the Archipelago, although possibly the cotton of these islands may be varieties of the species from difference of soil, climate, &c.

The best of the European cotton is brought from Cyprus; but Smyrna, Aleppo, Damascus, Jerusalem, &c. furnish likewise a quantity of cotton at least equal to the European.

Cultivation of Cotton in Persia.

THE annual cotton, or this last species, of which we have treated more amply, is much cultivated in the northern or colder provinces of Persia bordering on the Caspian Sea (as the perennial is in the southern); and it is from thence that the seeds now sent to Portugal have been obtained through the Bucharian Tartar merchants, and are the production of the *Gossypium herbaceum* of Linnæus, the *Gossypium annuum* of Pallas. It is sown in Persia from the end of March to the end of April, and reaped in September. This species requires a rich soil mixed with sand; and therefore where the land is not rich enough they

* The Antirrhinum minus, the Datura strantonium, the Gnaphalium americanum, &c.

† There is a kind of cotton cultivated in Malta, of a nankeen colour, which exceeds in fineness all other cotton, and is much superior even to that from the Antilles.

manure

manure it with cow or ſheep dung; although we are told that when the plants are once raiſed above the ground any ſpecies of ſoil will anſwer. The ground is worked in the ſpring, and the ſeeds are planted at the diſtance of eight or ten inches from one another, whilſt care is taken to weed it, to give air to the young plants. Dry ſummers give the beſt crop, as rain is more particularly hurtful when it falls in great quantities during the flowering and ripening of the cotton. It is gathered, as ſaid above, in September, care being always taken to collect a ſufficient quantity of ſeed for the next year. Laſtly, watering the young plants with a mixture of wood-aſhes and water in certain ſituations is ſometimes neceſſary to guard them from deſtructive worms.

The Ruſſians have cultivated the ſame ſpecies of Perſian cotton in the government of Caucaſus, and rear enough of it to ſerve their own national manufactures, which are not as yet either numerous or conſiderable; but on the Terck, at the foot of the Caucaſus, where it is reared, they do not ſow till the middle of May, leſt a late ſpring froſt, which is ſometimes felt in thoſe parts, ſhould deſtroy the hopes of the planter. With that one exception, the Ruſſians ſtrictly obſerve the Perſian mode of cultivation.

There is a ſpecies of ſilky cotton much cultivated at preſent in Germany, which poſſibly may merit the attention of Portugal for their plantations in America. It is the *Aſclepias ſyriaca* of Linnæus, and affords ſo fine a ſpecies of cotton (if I may ſo name it) that fabrics have been erected in Saxony, where ſtuffs are made of it which rival in luſtre, &c. the true animal ſilk. But this new vegetable ſilk has circumſtances attending it that ſeem to recommend its cultivation in ſome of the American colonies and iſlands: Firſt, becauſe it is originally the native of a hot climate, as Linnæus's ſpecific name indicates; and of courſe it is likely to be in its greateſt beauty and excellence in climates which approach neareſt to that of its native country. Secondly, becauſe its ſtalks afford a coarſe ſort of cloth well calculated to clothe negroes, whilſt from the pith of them paper is made.

VIII.

Facts and Obſervations concerning the Meaſure and Expence of firſt Movers, namely, Wind, Water, Steam, and Animal Strength, and on other Objects of general Utility. (*W. N.*)

THE conſideration of the value and importance of natural firſt movers is of conſequence not only to practical engineers, but to every individual in cultivated ſociety. There are numberleſs ſituations, even in the ſpirited manufacturing kingdom we inhabit, where large revenues are expended to perform mechanical and hydraulic operations by the force of human labour, or by horſes, which might be for the moſt part ſaved by ſubſtituting a ſteam-engine or windmill, or making uſe of a ſtream of water now running to waſte. It is well known, that, ſince the extenſion of the cotton and other works, eſtates of ſmall rent in the neighbourhood of Mancheſter and elſewhere have been, and continue to be, let at more than twenty times their original rent, merely from the fortunate circumſtance of their poſſeſſing a ſmall ſtream of water falling with a ſufficient declivity to give motion to a mill. If the proprietors of lands and manufacturers in general were better acquainted with the ſimple methods of eſtimating the forces of thoſe currents of water which run neglected through

through their grounds and premifes, and which an intelligent obferver need not walk many miles in any country to point out, their property and revenue might immediately receive a confiderable acceffion; and the community would be ftill more effectually benefited. The inceffant demands for the employment of fuch forces in grinding corn, colours, drugs, tobacco; in cutting bark and other tanners' and dyers' materials; in fawing wood; in laminating, drawing, or fafhioning metallic bodies; in fpinning, weaving, fulling, &c. the products of the organized kingdoms by arts already practifed, exclufive of the many improvements which may be expected in their application, are too numerous to afford the leaft reafon for any proprietor to fear a want of employment, or to confider the erection of a mill in a proper fituation as a fpeculation of the leaft danger or probable difadvantage. Similar obfervations are to a certain extent applicable in favour of the ufe of horfes inftead of men, and fteam inftead of horfes, in every cafe where the power is required to be great or long continued, and the fkill either little, or capable of being fupplied by machinery.

I am tempted to digrefs for a moment from my fubject by the natural recurrence of a political reflection, fo obvious that it fcarcely ever fails to be made when the extenfion of machinery and the application of inanimate powers are confidered. It is ftated by certain humane but miftaken objectors, that the fcheme of mechanical and chemical improvement is pointed againft the human fpecies, and tends to drive them out of the fyftem of beneficial employment. Two creatures offer themfelves to me for employment and fupport—a man and a horfe. I muft invariably prefer the latter, and leave the former to ftarve. Two other beings—a horfe and a fteam-engine, are candidates for my favour. My preference to the latter tends to exterminate the fpecies of the former. In both cafes it is ftated, that the number of intelligent creatures capable of the enjoyment of happinefs muft be diminifhed for want of fupport; and that, on the whole, the fum of the propofed improvements is not only a lefs proportion of good to fociety, but a pofitive acceffion of much mifery to the unemployed poor.

On this wide and extended argument, which can in fact be maintained againft improvements in no other way than by infifting that the favage ftate, with all its wants, its ignorance, its ferocity, and its privations, is preferable to the focial intercourfe of effort and divifion of labour we are habituated to prefer, it may be fufficient to obferve, that the topic includes matter not only for reafoning and induction, but alfo for experiment. By reference to the matter of fact, though it muft be allowed that new improvements, which change the habits of the poor, muft at firft expofe them to temporary inconvenience and diftrefs, againft which, in fairnefs, it is the duty of fociety to defend them; yet the invariable refult of fuch improvements is to better the condition of mankind. The nations which have fhewn the moft ingenuity and induftry in this way are not only the richeft, the moft populous, the moft intelligent, and the beft defended; but the provinces of thofe nations are feen to flourifh in proportion to their refpective degrees of activity in this refpect. And from thefe exertions it is, as Smith* emphatically remarks, that "the accommodation of an European prince does not always fo much exceed that of an induftrious and frugal peafant, as the accommodation of the latter exceeds that of many an African king, the abfolute mafter of the lives and liberties of ten thoufand naked favages."

* Wealth of Nations, i. ch. i.

But

But to return to our subject. In this practical and popular communication I shall avoid entering into any discussion of the theory of windmills, which has employed the attention of so many eminent men for a century past. The advantages of these engines for such work as will admit of being performed and suspended by intervals are sufficiently known, but perhaps not sufficiently attended to. There can be no doubt but that the Dutch, who use windmills for sawing, pumping, and various other works, as well as grinding, must have found them very profitable, since their country is overspread with them. It may be proper however to take notice, that many writers have copied one from another the determination of Maclaurin, that the best angle for windmill-sails to make with the line of direction of the wind is 54° 44′, which is only true, as that excellent mathematician observes, at the very commencement of the motion, and requires to be enlarged as the velocity of the sails increases: for the law of which, and other essential objects, his account of Sir Isaac Newton's Philosophical Discoveries, and his Treatise on Fluxions, may be consulted. Smeaton, who had much experience in the business of a civil engineer, and whose data may always be depended on, though his theories are not constantly accurate, made a set of experiments on the construction and effects of windmill-sails, which are described in the Philosophical Transactions for the year 1759*. This engineer states, that the mean rate of work for mills with Dutch sails is when they make about thirteen turns in a minute, which is when the velocity of the wind is $8\frac{2}{3}$ miles in an hour, or $12\frac{2}{3}$ feet in a second: and this wind in common phrase would be called a fresh gale. Taking the maximum of Desaguliers, hereafter to be mentioned, as his standard for computation, he deduces the size of a windmill-sail of the figure just mentioned, and also according to a figure constructed from his own experiments, which shall be equal in mean power to one man; and thence he arrives at the inference, that one of his own sails, thirty feet in length, will, when working at a mean rate, be equal to the power of 18,3 men. He had an opportunity of verifying this in the large way in a mill used for crushing rape-seed. The mean power of a windmill is therefore very considerable; but what may be the annual or average quantity of work such an apparatus is capable of performing under all the vicissitudes of the wind, I possess no means of ascertaining.

In the same treatise Smeaton makes several very just remarks on those windmills which are acted upon by the direct impulse of the wind against sails fixed to a vertical shaft. His objections have, I believe, in every instance been justified by the inferior efficacy of these mills when compared with the charges of erecting them. He also maintains that water-mills with oblique sails, upon the principle of the common windmill, cannot prove beneficial to the undertaker. It is indeed probable that most of the circumstances of running-water are likely to render the common over and undershot-wheels cheaper and more effectual, and that the oblique float-board will in no instance come near the effect of a close overshot wheel. But it is also certain that such wheels are used in China, in the south of France, and elsewhere, with much more effect than Smeaton appears disposed to think them capable of: and at all events, the subject of these wheels deserves to be considered.

* This account has since been republished, together with other valuable papers of the same author, under the title of An Experimental Inquiry concerning the Natural Powers of Wind and Water to turn Mills, &c. By John Smeaton, F. R. S. 8vo, printed for Taylor, London, 1794.

The enquiry which moſt immediately intereſts land-holders, and others who have the advantage of a current of water, is to aſcertain whether it will afford ſufficient power to juſtify the erection of a mill, and what that power may be. If the ſtream be ample without much fall, it muſt neceſſarily be applied to move an underſhot wheel by its impulſe, and the power will be determinable from the velocity of the water, and the quantity which paſſes through the ſection of its bed. One of the eaſieſt methods of aſcertaining theſe data is that given by Deſaguliers *, as follows. Obſerve a place where the banks of the river are ſteep and parallel, ſo as to make a kind of trough for the water to run through, and by taking the depth acroſs make a true ſection of the river. Stretch a ſtring at right angles over it, and at a ſmall diſtance another parallel to the firſt. Then take an apple, an orange, or other ſmall ball, juſt ſo much lighter than water as to ſwim in it, and throw it into the water above the ſtrings. Obſerve when it comes under the firſt ſtring by means of an half-ſecond pendulum, a ſtop-watch, or any other proper inſtrument, and likewiſe when it arrives at the ſecond ſtring. By this means the velocity of the upper ſurface, which in practice may generally be taken for that of the whole, will be obtained. The ſection of the river at the ſecond ſtring muſt be aſcertained by taking the depth as before. If this ſurface or ſection be the ſame as the former, it may be taken for the mean ſection; if not, add both together, and take half the ſum for the mean ſection. The area of the mean ſection in ſquare feet being then multiplied by the diſtance between the ſtrings in feet, will give the contents of the water in ſolid feet, which paſſed from one ſtring to the other during the time of obſervation. And this, by the rule of three, may be adapted to any other portion of time. Suppoſe, for example, the time had been 12″, and the hourly expenditure of water were required, the proportion would be: As 12″ are to 3600″, ſo is the number of cubic feet obſerved to the hourly expenditure in cubic feet. If the mere velocity be required in proportion to any fixed interval of time, the ſame proportion will give it, provided, inſtead of the ſolid contents in the third term, there be taken the diſtance between ſtring and ſtring.

The intelligent obſerver may in general abridge this operation, by taking notice of the arrival of the floating body oppoſite two ſtations on the ſhore, eſpecially when it is not convenient to ſtretch a ſtring acroſs. The arch of a bridge is a good ſtation for an experiment of this kind, becauſe it affords a very regular ſection and two fixed points of obſervation: and in ſome inſtances the ſea practice of heaving the log may have its advantages. Where a time-piece is not at hand it may be equally convenient, provided two obſervers attend, to note the time with a half or quarter-ſeconds pendulum. The half-ſeconds pendulum is made by ſuſpending a ſmall round button, or other ſpherical weight, by a thread looped over a pin of ſuch a length that the diſtance from the bend of the loop to the centre of the weight ſhall be 9,8 inches. The quarter-ſeconds pendulum is one fourth of this length. If, by obſervations at ſeveral ſtations above and below any particular point of the river, the velocity is not found to vary, the ſection of the river in all that ſpace may be concluded to be uniform; and it will not be neceſſary to determine more than one ſection by actual meaſurement.

In the caſe of an overflowing pond, or ſmall ſtream, which will admit of a dam acroſs it,

* Courſe of Experimental Philoſophy, ii. 419.

the

the quantity of water afforded may be aſcertained by ſuffering it to run through a notch in a board, or a vertical hole of an inch ſquare, according to the following table, which Deſaguliers ſays he calculated from repeated experiments*.

A TABLE of the Expence of Water through an Inch-ſquare Hole, and through a Cut an Inch wide and of different Depths.

Inches below the ſurface.	Expence of water through a hole of a ſquare inch at different depths, according to the number of inches in the firſt column. Tuns in an Hour.	Expence of water through a notch of different depths, according to the number of inches in the firſt column.
1	1,04	1,04
2	1,46	2,50
3	1,79	4,29
4	2,08	6,37
5	2,31	8,68
6	2,53	11,21
7	2,74	13,95
8	2,92	16,87
9	3,12	19,19
10	3,28	22,47
11	3,44	25,81
12	3,58	29,39
13	3,74	33,13
14	3,88	36,91
15	3,95	40,86
16	4,16	45,02
17	4,28	49,30
18	4,40	53,70
19	4,52	58,22
20	4,62	62,84
21	4,76	67,60
22	4,87	72,47
23	4,94	77,41
24	5,06	82,47
25	5,2, or 5 tuns and 50,4 gallons.	87,67

A cubic foot of water weighs very nearly $62\frac{1}{2}$ pounds averdupois, and an hogſhead of water weighs about 550 pounds.

* Courſe of Experimental Philoſophy, ii. 129.

In the conſideration of power or force to be derived from water in motion, the water may be taken as a determinate maſs falling through a given height in a given time. In order that this deſcending weight may cauſe another weight to aſcend, or may overcome ſome reſiſtance in the way of work with that degree of ſpeed which ſhall be the moſt profitable, it is neceſſary that the reſiſtance or work to be done ſhould be neither too great nor too little. If it be too great, the ſlowneſs of operation will diminiſh the quantity of work; and if it be too ſmall, the ſpeed will not ſufficiently compenſate for this ſmallneſs. When the power is therefore known, it remains to deduce what may be the effect. But in the firſt place, as the height from which the water flowing in a river may have deſcended, in order to acquire its velocity, is, from a variety of circumſtances, difficult to be aſcertained, and alſo very different from that height which would immediately and without impediment produce the ſame velocity, it becomes neceſſary to compute this laſt height, which hydroſtatical writers uſually call the height of the virtual head. Theſe writers teach, that the velocity of a fluid ſpouting through an orifice in a thin plate is the ſame as would be acquired by a body falling in clear ſpace from the height of the ſurface of the fluid above the orifice. Hence, from the common doctrine of falling bodies, if the uniform velocity of a ſtream be expreſſed in feet per ſecond, the virtual height of the fall will be found by multiplying the given velocity into itſelf, and dividing the product by 64,2882; the quotient will be the required height expreſſed in feet *.

The effect of underſhot and overſhot-wheels has been treated by various authors, who have given reſults extremely different from each other. Smeaton, in the Treatiſe often quoted in the courſe of this communication, obſerves, that Belidor in his Architecture Hydraulique, i. 286, endeavours to demonſtrate that water applied underſhot will do ſix times more execution than the ſame applied overſhot; while Deſaguliers, whom he (Smeaton) miſquotes by overlooking the difference of fall, is ſaid to have given the advantage as ten to one in favour of the overſhot †. The particular experiments of Smeaton himſelf, as well as his experience, point out the following reſults.

The effect in underſhot-mills in the large way is at beſt one third of the power; that is to ſay, the wheel, being driven with two-fifths of the velocity of the ſtream, will raiſe a quantity of water equal to one-third of the column, which ſtrikes the float-boards, to an height equal to that of the virtual head or fall: and the effect of an overſhot-wheel will be, at a medium, twice that of the underſhot. Mills having a breaſt-wheel, or other kind of wheel on which the water acts, partly by its weight and partly its impulſe, will produce more or leſs effect, accordingly as the circumſtances approach more nearly to thoſe of the over or underſhot-wheels.

For the advantage of ſuch as are leaſt converſant in ſubjects of this nature, for whom chiefly the preſent memoir is intended, I ſhall illuſtrate the ſubject by an example.

Suppoſe a ſtream to paſs through an eſtate without any evident fall, with a velocity of nine feet per ſecond, and affording ſufficient room to place an underſhot-wheel with a pro-

* The proof of this is ſimple, but may alſo be ſeen in Deſaguliers, ii. 520.—Thoſe who uſe logarithms may with leſs trouble *ſubtract the conſtant log.* 1.8081312 *from twice the log. of the velocity, and the remainder will be the log. of the virtual height.*

† The proportion of work of the two actual mills, compared by Deſaguliers, is as 3,25 to 1 in favour of the overſhot.

per

per number of float-boards, each ſix feet long and two deep: It is required to determine, 1ſt, What quantity of water might be raiſed by that power, during the day of ten hours, to the height of thirty feet? or, 2dly, What number of buſhels of wheat, or malt, might be ground in the ſame time? or, 3dly, What number of men or horſes might this wheel ſupply the place of, in performing any other kind of work or manufacture?

In the firſt place, to find the virtual fall, multiply 9 feet by itſelf, which gives 81, and divide this by 64,2882: the quotient will be 1,26 feet.—Again, the ſurface of the float being 6 feet by 2 feet, is 12 ſquare feet; which, multiplied by the velocity 9, gives 108 = the number of ſolid feet of water which has in effect fallen from the virtual head in a ſecond. But this power will partly be conſumed in producing eddies and lateral motions, and partly remain uncommunicated in the tail water as it flows off. For which reaſons, as has already been remarked, the effect will only be one-third of the power. That is to ſay, one-third part of 108, or 36 cubical feet of water will be raiſed in a ſecond to the height of 1,26 feet. The queſtion however is directed to the height of thirty feet, and the quantities of water, or other weights, raiſed by equal powers, are inverſely as their heights: that is, 30 feet : 1,26 feet :: 36 cubical feet : 1,512 cubical feet raiſed 30 feet in a ſecond. But the hour conſiſts of 3600 ſeconds, and the day of ten hours, = 36000, which multiplied by 1,512 produces 54432 cubic feet, or 6437 hogſheads, (reckoning the gallon at 231 ſolid inches) which are raiſed 30 feet in the day *.

With regard to the operation of grinding, it is allowed that about 3420 cubic feet of water with a fall of 10 feet will, by an overſhot wheel, grind one buſhel of wheat into flour, and the ſame force will cut five quarters of malt. But our underſhot wheel will only do half the work. From the foregoing calculations our ſtream is equivalent to 13,6 ſolid feet per ſecond with a fall of ten feet, or 816,5 ſolid feet per minute. Therefore as 3420 cubic feet : one buſhel of wheat :: 816,5 cubic feet : 0,239 of a buſhel which would be ground per minute by an overſhot wheel; or 0,1195 by our underſhot wheel. This laſt number multiplied by 60 produces 71,7 buſhels per hour, or nearly 72 buſhels, or 9 quarters of wheat per day of ten hours. And by the proportion of malt to wheat juſt mentioned, it will follow that 360 quarters of the former grain might be cut in the ſame time.

Animal ſtrength is of ſo fluctuating a nature, that it is difficult to ſubject it to any eſtimate. Phyſical cauſes muſt affect both the quantity and duration of the efforts

* This is an outſide eſtimate, and gives no allowance for frictions and errors in the conſtruction of the hydraulic work. The reaction in Smeaton's Experiments was afforded merely by a weight and pulley, which is greatly in favour of the mill in deducing the effect. I find in the "Reports of the late Mr. John Smeaton, F. R. S." printed in quarto, London 1797, the power of the water called "Hubbert's mill ſtream" is deduced from the quantity and fall, (p. 245.) by a rule which he does not mention, but is evidently this, for overſhot or cloſe breaſt wheels: *Multiply the number of cubic feet of water diſcharged per minute by the fall in feet: this number may be called the power. Divide the power by two, and the quotient may be called the effect. Aſſume any height at pleaſure in feet: divide the effect by the height, and the quotient will be the cubic feet of water, which a good hydraulic apparatus will raiſe to that height per minute.*

The underſhot wheel will raiſe half that quantity: or in general leſs than one-ſixth of the power.

I have retained the proportion of the Treatiſe on mills in the text, becauſe the ratio of the effect to the power can be eaſily altered at pleaſure in computing.

poſſible

possible to be made either by man or beast, and the former is more particularly influenced by his moral habits. From these last it is that the influence of reward, the expectation of favour or patronage, and various other similar motives, have operated in the temporary exhibition of hydraulic machines, to produce results contrary to every sound deduction from permanent work, and most perniciously delusive to the parties concerned in supporting or encouraging such engines. Desaguliers*, who has taken much pains to ascertain the maximum of power in this respect, has determined that a man can raise of water, or any other weight, about 550 lbs. or one hogshead ten feet high in a minute; and he states that a horse will raise five times that quantity; or, which is the same thing, that quantity through five times the height. His deduction respecting the man, though he says it will hold good for six hours, appears from his own facts to be too high, and certainly such as could not be maintained one day after another. Smeaton † considers this work as the effort of haste or distress. He reports ‡ that six good English labourers will be required to raise 21141 solid feet of sea water to the height of four feet in four hours. This quantity is of the same weight as 21669 cubic feet of fresh water, with which, and the above rate, it will be found by an easy calculation that the men will raise a very little more than six cubic feet each to the height of ten feet in a minute. But the hogshead containing $8\frac{1}{2}$ cubic feet, Smeaton's allowance of work proves less than that of Desaguliers in the proportion of 6 to $8\frac{1}{2}$. And as his good English labourers, who can work at this rate, are by him estimated to be equal to a double set of common men picked up at random; it seems very proper to state that, with the probabilities of voluntary interruption, and other incidents, a man's work for many days together ought not to be estimated at more than half a hogshead raised ten feet high in a minute. In the same report last quoted, Smeaton states, that two ordinary horses will do the work in three hours and twenty minutes, which amounts to a little more than two hogsheads and a half § raised ten feet high in a minute. One horse will therefore do the work of five men.

To apply these deductions to our example, it must be recollected that the quantity raised in ten hours to the height of thirty feet was inferred to be 6437 hogsheads, which are equivalent to 32 hogsheads raised ten feet high per minute. Consequently at the rate of one man for each half hogshead, the stream would perform the work of 64 men, or nearly thirteen horses.

With regard to streams which fall by a considerable declivity, the water may be conveyed by the well known means of a dam or trough to the buckets of an overshot wheel, placed in that part of the stream which is found the most convenient in point of expence and local situation. Suppose, for example, the fall amounted upon the whole to fifteen feet, upon a length of 200 yards, it might be more convenient to lead the whole stream in a wooden trough supported upon posts, with a slight declivity, to the wheel near the lower end of the current; or in other circumstances, according to the face of the land, the work might prove cheaper or more durable if the wheel were placed near the upper end of the stream, and the channel sunk so as to convey away the tail water with no more fall than should be necessary for that purpose. In this case it is obvious that the fall must be ascer-

* Course of Lectures, ii. 498, 505, 536. † Reports, i. 216. ‡ Ibid. i. 323.

§ He elsewhere (ibid. p. 229.) rates an horse at 250 hogsheads, ten feet high, in an hour; but I prefer the deduction in the text.

tained by the operation of levelling, and not by computation as in our former supposition. This operation is easy, and may be performed upon such short distances with sufficient accuracy, with a carpenter's square and a staff.

For this purpose it will be necessary to drive a pin into the handle, or thickest part of the square, near the corner, upon the flat side. A line must be drawn from the pin, parallel to the edge of this handle, and a looped thread supporting a small weight must be hung upon the pin. In this situation the instrument is ready for taking levels. For if it be held in such a position that the plumb line may cover the line drawn from the pin, the blade will lie horizontal, and by looking along its upper edge the eye will be directed to some object on the same level with itself. Let the observer provide himself with a staff five or six feet long; which, if divided into feet and inches, will be the more useful. This is to be pitched at or near the lower end of the stream, and against it the observer is to press the handle of his square, which may be conveniently done by grasping both it and the staff together in the same hand. Then, holding the apparatus so that the plumb line may lie fair, he is to direct the blade of the square to some stone or other remarkable object higher up the stream; for which purpose it will be necessary to slide the handle of the square up or down the staff till the positions are accurately obtained; that is to say, till the blade points fairly to the object at the same time that the plumb line denotes that the blade is level. When this is done, a memorandum must be taken of the feet and inches from the bottom of the staff to the upper edge of the blade, and the observer must proceed to the stone or object to which his sight was directed, and, planting his staff there, repeat the same operation with regard to some other object still higher up the stream, and again note down the feet and inches observed upon the staff. This process continued will, by a few reiterations, bring him to the upper extreme of the water which we suppose to be within his grounds or power. The sum of all the measures taken upon the staff is the difference of level or whole fall of the water. The quantity of water in cubic feet may be ascertained by either of the means before mentioned; or, if the stream be very small, it may actually be dammed, and measured off with two tubs or measures, of as large a size as can conveniently be managed.

I should be inclined to apologize to such of my readers as are intimately acquainted with these subjects, for the minuteness of detail into which I have entered, if I were not assured that they will be the first to perceive, that many individuals who possess beneficial streams are totally uninformed of their value, or of the means of determining the same.

(To be continued.)

IX.

Enquiries concerning the Invention and Practice of the Art of Hat making.

To Mr. NICHOLSON.

Newcastle, 15th Dec. 1798.

SIR,

YOUR Publication being open to enquiries relative to the advancement of manufactures, as well as the sciences, will you favour me with a place for the following queries, on

on a buſineſs but little known? And probably, amongſt your ſubſcribers, they may be anſwered through the ſame medium.—How, and at what time, was hat-making invented, and by whom?—Were there ever any engines made uſe of in that buſineſs?—Are there any at preſent? And if ſo, in what part of the country are they made uſe of? And how far do they go in the proceſs?—Are there any machines for the cutting of hare, rabbit, or beaver ſkins? And if any, where are they worked?—Your inſerting the above in your next number will oblige, Sir, yours,

N. L.

There is no mention made of the above, either in the Tranſactions of the Royal Society of London, or in Beckman's Hiſtory of Inventions.

THE above did not come to hand early enough for me to make any enquiries reſpecting the invention, hiſtory, and practice of the art of hat-making. Reſearches of the kind pointed out by this correſpondent are peculiarly adapted to the views and intention of a Journal of the Arts; and I hope he will not be diſappointed in his expectations from others of my readers, whoſe purſuits may have enabled them to throw light upon the ſubject. At all events, I ſhall certainly have ſome information to communicate in the next number, which, for want of time, I cannot at preſent arrange and digeſt.

W. N.

X.

New Obſervations on the Method of producing very loud Fulminations with various Bodies, by Means of Phoſphorus. By Cit.* BRUGNATELLI.

I WAS aware that the oxygenated muriate of potaſh produces effects ſuperior to nitre, when mixed with charcoal and ſulphur and converted into gunpowder; that it detonates by percuſſion or trituration with a great number of combuſtible ſubſtances; but I did not expect to produce effects much more conſiderable by uſing the ſimple nitrates, and even the metallic oxides mixed with phoſphorus and ſtruck with a hammer.

Experiment 1. I took a gros of the cryſtallized nitrate of ſilver, which I placed on an heavy anvil, and laid a very thin ſlice of phoſphorus in the midſt of the cryſtals. The temperature of the atmoſphere during theſe experiments was not higher than 6° above the zero of Reaumur's thermometer. The materials being thus diſpoſed, I ſtruck the mixture rather ſmartly with a hammer. The conſequence was one of the moſt terrible detonations I ever witneſſed, which ſhook the anvil and its wooden ſupport. Streaks of ſilver were obſerved upon the anvil five or ſix lines in length. One of the edges of the hammer was bended and turned upwards. I was perfectly ſtunned, and my clothes were torn in various places.

* Tranſlated by Van Mons from the Italian manuſcript of the author into French, and inſerted in the Annales de Chimie, xxvii. 72.; from which work the preſent tranſlation is made.

I have

I have frequently repeated the ſame experiment, even in my lectures; and though I operated with no more than a few grains of the ſalt, the noiſe of the exploſion was never leſs than that of a muſquet.

The lapis infernalis has nearly the ſame effect as the nitrate of ſilver.

Experiment 2. I placed on an anvil a pinch of the oxygenated muriate of potaſh with a ſmall portion of phoſphorus, and ſtruck the mixture with a hammer. The detonation was exceſſively ſtrong.

Experiment 3. The dry nitrate of biſmuth detonated very ſtrongly. The ſame effect was produced with all the metallic nitrates which were to be found in the laboratory, and particularly with the nitrate of tin.

Experiment 4. I repeated the ſame experiment with the fuſed nitrate of mercury, of which I put ſix grains with a ſmall quantity of phoſphorus on an anvil, and ſtruck them with a hammer. The phoſphorus ſimply took fire, without producing any noiſe: but when the hammer was ſlightly heated, the ſame mixture detonated with a ſhocking noiſe. After the fulmination the mercury was found reduced, having, as it were, ſilvered the anvil in very brilliant radiations.

Experiment 5. I afterwards tried the alkaline nitrates, particularly that of potaſh. When a ſmall ſlice of phoſphorus was laid on a pinch of nitre and ſtruck with a cold hammer, no fulmination was produced even by repeated blows; but having ſlightly heated the hammer, in order that the affinities might act more deciſively, the very firſt blow produced a very loud fulmination, incomparably ſtronger than that of gunpowder.

Experiment 6. I obtained no fulmination or detonation with the ſulphates of alumine, of copper, or of iron ſtruck with phoſphorus in the before-mentioned manner, though I heated the hammer more than uſual.

Experiment 7. Neither did I obtain any fulmination from the ſimple muriate of ſilver, commonly called luna cornea, by the ſame treatment with phoſphorus.

Experiment 8. I likewiſe tried the oxygenated muriate of ſilver, obtained by decompoſing the nitrate of ſilver by the oxygenated muriatic acid. A pinch of this ſalt (with phoſphorus), ſtruck with a warmed hammer, produced a much weaker fulmination than was obtained with the cryſtallized or fuſed nitrate of ſilver, or the other ſalts before mentioned. The oxygenated muriate of mercury, treated in the ſame manner, afforded a very weak detonation.

Experiments 9 *and* 10. I endeavoured to produce fulminations with the metallic oxydes treated with phoſphorus. Thoſe of manganeſe, of zinc, of copper, of iron, of antimony, and of lead, produced no effect, even when ſtruck with the hammer conſiderably heated; but I obtained fulminations with the yellow oxyde of mercury (turbith mineral) and the grey oxyde of the ſame metal.

Turbith mineral does not fulminate in contact with phoſphorus when it is pulveriſed. It muſt be in a lump to produce this effect. The ſame thing happens with regard to the grey oxyde of mercury by the nitric acid, except that the fulmination is ſtronger.

Experiments 11 *and* 12. I was curious to aſcertain whether I ſhould obtain ſimilar fulminations with the ſalts before mentioned, by ſubſtituting another acidifiable combuſtible ſubſtance inſtead of phoſphorus; as, for example, ſulphur and charcoal.

I accordingly took nine grains of lapis infernalis and three grains of pulveriſed ſulphur, which

which I ſtruck with the cold hammer. The ſulphur took fire without affording any noiſe; but, when the hammer was uſed hot, a detonation was heard, and rays of ſilver appeared on the anvil.

Having repeated the ſame experiment with lapis infernalis and charcoal, I could produce no more than a very dull detonation, though the hammer was well heated.

Experiment 13. I took ſeveral of the ſalts which had fulminated by the ſtroke, and threw them, together with the phoſphorus, into the liquid oxygenated muriatic acid; but no detonation was produced. In an atmoſphere of the gas of the ſame acid, the fulminating mixtures burned with a ſlight crepitation, ſimilar to that which is excited by phoſphorus alone.

Experiment 14. I wrapped moſt of theſe detonated mixtures in ſmall pieces of paper, and threw them one after the other into a red-hot crucible. They burned with a very lively flame, but did not fulminate nor detonate.

Conclusion.

1ſt. THE nitrate of ſilver, whether cryſtallized or fuſed, fulminates when ſtruck with a hammer, together with phoſphorus, even at a low temperature. *Exp.* 1.—All the ſalts of ſilver do not fulminate equally, nor in the ſame manner. *Exp.* 7.

2d. Moſt of the metallic nitrates fulminate with phoſphorus. *Exp.* 3 and 4.

3d. Common nitre, of which the fulminating property was before known, in its mixture with various combuſtibles, elevated to a certain temperature, or put into contact with an inflamed body, as in gunpowder or fulminating powder, is now found to detonate with phoſphorus alone. *Exp.* 5.

4th. Thoſe ſalts into which the nitric acid has not entered as a component part do not fulminate.

5th. The oxygenated muriates of potaſh, of ſilver, and of mercury, fulminate with phoſphorus, but the latter with much leſs effect than many other ſalts. *Exp.* 8 and 2.

6th. The ſalts are not the only bodies which fulminate with phoſphorus: ſeveral metallic oxydes have the ſame property. *Exp.* 9 and 10.

7th. Phoſphorus likewiſe is not the only acidifiable and ſolid combuſtible matter capable of producing fulminations. Charcoal produces the ſame effect at a more elevated temperature. *Exp.* 11 and 12.

8th. Thoſe ſubſtances which fulminate with phoſphorus produce no effect when thrown into the liquid oxygenated muriatic acid. *Exp.* 13.—Neither do they fulminate when expoſed together with phoſphorus to an elevated temperature. *Exp.* 14.—The blow of the hammer is neceſſary to throw the component parts of theſe bodies into a ſtate of oſcillation, in order to determine the affinities with effect.

ADDITION BY VAN MONS.

THE facts obſerved by the learned editor of the Italian Annals of Chemiſtry appeared too curious and important for me to loſe any time in repeating his experiments. My ſucceſs was as follows:

The

The cryſtalliſed nitrate of ſilver detonated very ſtrongly, emitting only a weak flame. The ſilver was reduced to the ſtate of a blackiſh oxyde.

Lapis infernalis alſo detonated, but much more weakly. The metal was completely reduced.

The nitrate of tin fulminated with conſiderable ſtrength.

The fuſed nitrate of mercury produced a ſtrong effect.

The different oxydes tried by Brugnatelli, that of manganeſe excepted, produced no noiſe by friction; and the detonations under the ſtroke were at leaſt doubtful. The oxydes were all in part reduced, and the mixtures burned in a lively manner, throwing out ignited ſparks to a diſtance; but the phoſphorus alone, after three or four ſtrokes with the hammer, exhibited the ſame appearance.

The oxygenated muriate of mercury afforded perceptible marks of detonation.

I produced theſe ſeveral fulminations without heating the hammer for any one of them. Friction, or a few ſlight previous blows, heat the materials ſufficiently to cauſe the effect to take place with the laſt ſmart blow.

I tried a great number of times to make the experiment with ſulphur and lapis infernalis. In ſome inſtances I rubbed the two ſubſtances together with all my force in iron or marble mortars, and in others I ſtruck them with a heated hammer; but I did not obtain the moſt feeble detonation. The matter itſelf did not even take fire, excepting when I ſtruck it ſmartly in an iron mortar ſtrongly heated with a peſtle equally hot. I was not more fortunate with charcoal. Similar experiments, which I have ſince tried with various other ſubſtances, afforded me the following reſults:

The nitrate of gold by evaporation produced a louder detonation than is afforded by the oxygenated muriates of alkali. The marble mortar in which I made the experiment was covered with ſmall particles of exceſſively thin plates of reduced gold. The metal ſeemed to have undergone a very liquid fuſion.

The muriate of mercury detonated with a force at leaſt equal to that of the nitrate of gold. The metal was almoſt entirely diſſipated.

The nitrate of lead produced a weaker detonation than the foregoing. The metal was found reduced to the ſtate of black oxyde.

The oxygenated muriates of antimony, zinc, and tin, produced a much weaker effect. I ſucceeded, however, once in producing a very ſtrong and inſtantaneous detonation with the firſt of theſe ſalts.

The oxydes of gold, ſilver, and mercury, by fire, were found to occupy the firſt rank among the fulminating ſubſtances. The oxygenated muriate of potaſh does not afford effects equally conſtant with thoſe of the oxydes.

The oxyde of lead, in its different degrees of oxydation, afforded in no inſtance any appearance but that of inflammation.

I formerly exhibited in my public lectures the detonation which may be produced by percuſſion in a mixture of the nitrate of potaſh and phoſphorus. I had even produced exploſions ſufficiently violent to induce the audience to ſuppoſe that I operated with the oxygenated muriate. I ſubmitted the nitrate of ammoniac to the ſame treatment, and produced a detonation ſo terrible that it ſhook the doors of my laboratory.

The nitrates of barytes, of ſtrontian, and of magneſia, by deſiccation, detonated with nearly the ſame force as the nitrate of potaſh.

I had ſtill remaining five grains of the oxygenated muriate of ammoniac, of which I took the half, with about four grains of phoſphorus. I ſlightly cruſhed theſe two ſubſtances, with the intention of mixing them together, when a detonation ſo terrible was heard that the whole houſe was alarmed. The phoſphorus was for the moſt part thrown upon my hat, which it burned for a long time before I perceived it. The violence of the blow cauſed the hammer to fly out of my hands. I very diſtinctly felt it raiſed by the expanſion of the gaſes. The other half of the oxygenated muriate of ammoniac was placed alone upon the bottom of an inverted iron mortar, and ſtruck rather briſkly with the cold hammer. The firſt blow cauſed it to detonate, but no light was diſengaged. I regretted that no more of this ſalt remained to make the experiment with ſulphur and charcoal.

I alſo ſtruck the nitrate of ammoniac alone. The fourth blow cauſed it to fulminate. I ſay it fulminated, becauſe the detonation was accompanied with a diſengagement of light.

I obtained an effect equally conſiderable from the white nitrate of mercury and ammoniac. I think that all the nitro-ammoniacal triple ſalts will detonate more or leſs ſtrongly with phoſphorus.

I was deſirous of trying the ſolid oxygenated muriatic acid; for which purpoſe I cauſed this acid to cryſtallize by artificial cold. A few cryſtals of this acid were put together, with a ſmall piece of phoſphorus, upon an iron mortar placed in a cooling mixture. When theſe were ſtruck with a hammer, a dull detonation was heard at the ſixth or ſeventh blow. The fuſed acid was projected to a diſtance.

I repeated a great number of theſe experiments with pyrophori and the phoſphori called artificial carefully prepared; and in almoſt every caſe I obtained a detonation. I ſhall deſcribe my experiments on theſe ſingular inflammable preparations in a ſeparate article.

Whenever I ſtruck the before-mentioned mixtures with a heated hammer, I always obtained a weaker detonation, at the ſame time that the inflammation was ſtronger; and when the hammer was too hot, no detonation or noiſe was produced. This obſervation I had already made with the oxygenated muriate of potaſh; and it appears to me to explain the phenomena of detonation. The blow with the cold apparatus, by ſtrongly compreſſing the matter, and perhaps exciting ſome heat, produces a partial combuſtion of the phoſphorus, and conſequently engages only part of the oxygen of which the other portion aſſumes the elaſtic ſtate, and produces the noiſe of the detonation. At a very elevated temperature the effects are not altogether the ſame. All the oxygen is at once employed to burn the combuſtible, whence the inflammation is the ſtrongeſt, and no detonation takes place. In fact it is obſervable, that the more ſudden and ſonorous the noiſe, the leſs is the developement of fire; and the contrary. To this principle alſo is referable the obſervation made by Brugnatelli, which I have found to be true, that the oxygenating body muſt in ſome inſtances poſſeſs the form of a lump to obtain the effect; becauſe it applies too extenſively to the combuſtible matter when powdered.

Theſe experiments ſucceed much better by friction than by a blow, and the effects take place more readily upon a rough than a ſmooth body. I uſually place the two ſubſtances on the bottom of an inverted marble mortar, which is left rough from the ſaw.

Upon

Upon these I press the face of an iron hammer, holding the handle with the left hand, and suddenly slide the hammer forward so as to produce a kind of shock. In this manner the detonation or noise is likewise more distinct, and its force more easily determined. Simple compression produces the detonation in many cases.

I think it proper to warn those who are not familiar with experiments of this kind, that when the operation is performed with more than a grain and a half, or at most two grains of phosphorus, there will be danger of burning their hands or clothes by the excess of this combustible, which flies off in a burning state.

I cannot finish this article without restoring to its true proprietor the portion of honour which is due to the discoverer of the detonating property of the oxygenated muriate by percussion or strong friction, which Citizen Fourcroy has attributed to me in the Memoir * he has communicated to the National Institute, no doubt in consequence of my having mentioned this phenomenon in my edition of the Philosophy of Chemistry, under the article of *Oxygenated Muriates*. Professor Wurzer, of Bonn, was the first who observed this fact, by triturating with some force, in a mortar, a mixture, scarcely weighing a grain and a half, of three parts of the oxygenated muriate of soda and one part of sulphur. He obtained a detonation which rendered him deaf for several days. See Crell's Chemical Annals for the year 1792, vol. ii. page 402.

TUNNEL BENEATH THE THAMES.

Reports, with Plans, Sections, &c. of the proposed Dry Tunnel or Passage from Gravesend in Kent to Tilbury in Essex, demonstrating its Practicability, and great Importance to the two Counties and to the Nation at large; also, on a Canal from near Gravesend to Stroud, with some Miscellaneous and Practical Observations. By R. Dodd, Engineer. Quarto. 28 pages, with 3 plates: viz. 1. Plan and Sections of the proposed Tunnel. 2. A View of Gravesend and Tilbury, with the Section of the River, shewing the Strata and Depth of Water: and, 3. A Map of the Country within twenty Miles of Gravesend. London, printed for J. Taylor, 1798.

SOME account of this undertaking was given in this Journal a few months ago. (II. 239.) The present Reports will afford satisfactory information of its detail and progress, and the public will hear with pleasure that it is likely to be carried into effect.

In the month of May 1798, Mr. Dodd circulated an Introductory Report or Address to the Nobility, Gentry, &c. of Essex and Kent, in which he states that the extended scale of commerce on the river Thames forbids the construction of a bridge at Gravesend; but that the practice of making passages, tunnels, or drifts under rivers has been adopted to a great extent in various parts of the kingdom; namely, at the coal works under the rivers Tyne and Wear, and at Whitehaven under the very ocean; that a tunnel is actually intended to be made under the mouth of the river Tyne, to answer the purposes of a bridge for carriages and passengers;—that the measure now proposed will save a circuit of near fifty

* Philosophical Journal, I. 168.

fifty miles to carriages paſſing from one county to the other; and that the ſituation at or near Graveſend is peculiarly eligible from its natural facilities, as the greateſt part of the ground to be paſſed through is chalk. Theſe obſervations are followed by the propoſal and eſtimate which have already been given at the page of our Journal laſt quoted. Mr. Dodd obſerves that 12 l. per running yard muſt be conſidered as a handſome allowance, ſince it is a fact, that ſome of the moſt conſiderable tunnels in this kingdom on a ſcale of nineteen feet by ſeventeen, which is larger than the tunnel at preſent recommended, have been executed at that price.

On the 18th of July 1798, a meeting was held in the Town Hall of Graveſend, the Earl of Darnley in the chair; when it was reſolved, That there being no reaſon to doubt the practicability of ſo deſirable a meaſure, the co-operation of government, the aſſent of the neighbouring land-owners, or the willingneſs of the public to ſubſcribe to its accompliſhment, the opinion of the meeting was, that transferable ſhares of 100 l. each would afford the beſt means of effecting it; and that a ſubſcription ſhould be opened at the office of Meſſrs. Evans and Son, at Graveſend, for its ſupport and encouragement.

In the report and eſtimate which the engineer preſented to this meeting, he ſtates, that from the actual ſurvey he finds no reaſon to add to the former eſtimate, but rather to abate ſomewhat more than 3000 l. on account of the excellent chalk which he finds will be afforded at the moſt eligible place for the excavation. The deviation from an horizontal line of the tunnel, which paſſes at about thirty feet below the bed of the river, will be four inches in the yard. Several military and commercial advantages of the undertaking are likewiſe clearly ſtated, and an eſtimate given of the coſt of a twelve feet tunnel, admitting carriages to paſs only one way at a time, in caſe the ſixteen feet tunnel ſhould be thought too expenſive. In the ſubſequent obſervations the author enlarges ſtill more upon the ſame intereſting topics, and makes a compariſon between the communication by tunnels and the great modern improvement of iron bridges, which laſt erection he ſhews to be much leſs durable, and more liable to ſpeedy deſtruction by an enemy, than the tunnel. He remarks, that the deſtructive elaſtic fluids, which are known to be extricated in mines, cannot affect the preſent work when once finiſhed, becauſe of the clear communication with the external air at both ends; and he gives ſome practical remarks on the diſcovery of inflammable air or fire-damp, which, on account of the intereſting nature of the ſubject, I ſhall here tranſcribe.

"On entering any excavation under ground, where this fire-damp is ſuſpected to lie, the candle ought to be held in the left hand (the ſmaller the candle the more to be depended upon), and the flame thereof to be ſhaded by the right-hand fingers being placed horizontal, and on a level with the top of the flame of the candle. If the air be good, and perfectly free from any inflammable air or hydrogen, a ſmall brown top or pinnacle is ſeen on the top of the flame, the ſame as is ſeen in the chamber of a houſe; but on approaching the real fire-damp another top or ſpire is ſeen of a blue colour, above the brown juſt deſcribed as ariſing therefrom; and on advancing a ſtep or two forward the damp is commonly more denſe, and conſequently the appearances over the candle aſſume a more ſerious aſpect by the brown ſpire or ſmall top beginning to diſappear; another ſtep perhaps reduces it altogether, on which the danger commences; and the ſpire or top ſits down altogether blue on the flame. At this laſt ſtage it is not ſafe to proceed further without great

great circumſpection joined with ſteadineſs: a little further the blue top is ſeen to be more perfect and denſe, which if general through the place, all lights ſhould be extinguiſhed, and the workmen be made to quit the place. Perhaps on advancing a very little further a light blue bead appears to circumſcribe the other dark blue, as if approaching to flame, and having a more than ordinary pointed ſpire terminating like a white thread; and the danger becomes very great if to remain any time in this poſition. The next ſtage ſhews the ſprents to have taken place, which ſprents are ſimilar to thoſe produced by ſqueezing the oil from the rind of a lemon into the flame of a candle: at this period ſometimes has been obſerved a ſmall buſhy dark-coloured cloud hanging over the top of the flame, ſtill more and more attracting the fire; when in an inſtant perhaps after this laſt obſervation the whole appearance expands into flames, and one general conflagration takes place. The miſchief which enſues is in proportion to the accumulated quantity of this pernicious vapour; which if great flies to the neareſt aperture leading to the atmoſphere, being about eleven times lighter than common air, with a dreadful exploſion and extraordinary eruption. But in caſe the ſaid fire-damp or inflammable air be mixed with ſtyth, or black damp, theſe appearances are ſomewhat varied; and by being mixed with a very dark brown is in general more ſafe.

"Black damp or ſtyth" (doubtleſs carbonic acid, or hydro-carbonate) "which ariſes in mines is directly the reverſe of inflammable air or fire-damp; the former extinguiſhes the flame of a candle as quickly as if put into water or any other fluid; the other cauſes the candle to burn too faſt. This ſtyth or black damp prevails moſt where there is little or no inflammable air, and when the due circulation of atmoſpheric air is neglected."

It was of eſſential conſequence, by way of ſhewing the practicability of the preſent undertaking, that Mr. Dodd ſhould impreſs the minds of his employers with the fact that ſuch tunnels had been made with ſucceſs on a much larger ſcale. For this purpoſe he relates various hiſtorical matters, beſides thoſe already mentioned, from which we may notice, that the earlieſt tunnel for inland navigation was executed by M. Riquet, to convey the canal of Languedoc through a mountain near Bezieres. The firſt executed in this country was by the celebrated Brindley on the duke of Bridgewater's canal near Mancheſter. The next is the famous tunnel of Hare Caſtle Hill, in Staffordſhire, by the ſame engineer, which is 2880 yards long, and paſſes through a variety of ſtrata, quickſands, &c. more than ſeventy yards below the ſurface of the earth, and ſerves as the receptacle for part of the Grand Trunk Canal. The tunnel of Saperton is two miles and three quarters long, and was carried through two miles of ſolid rock. Many other drifts or tunnels have been made in this kingdom, through rocks and obſtacles of various kinds, in a courſe of time and at expence much leſs than would be apprehended by perſons unacquainted with works of this nature.

Mr. Dodd's report and eſtimate on the projected dry tunnel under the river Tyne contains illuſtrations and argument of nearly the ſame import. His pamphlet concludes with a propoſal for an inland canal from the Thames near Graveſend, to the Medway near Stroud; which, by a ſhort courſe of about ſix miles, would ſave the circuitous and leſs certain paſſage of forty-ſeven miles by the way of the Nore. The peculiar advantages of this cut to the country at large, and to the government eſtabliſhments on theſe rivers, as well as the nature of the ground itſelf, are ſtrongly and perſpicuouſly ſtated.

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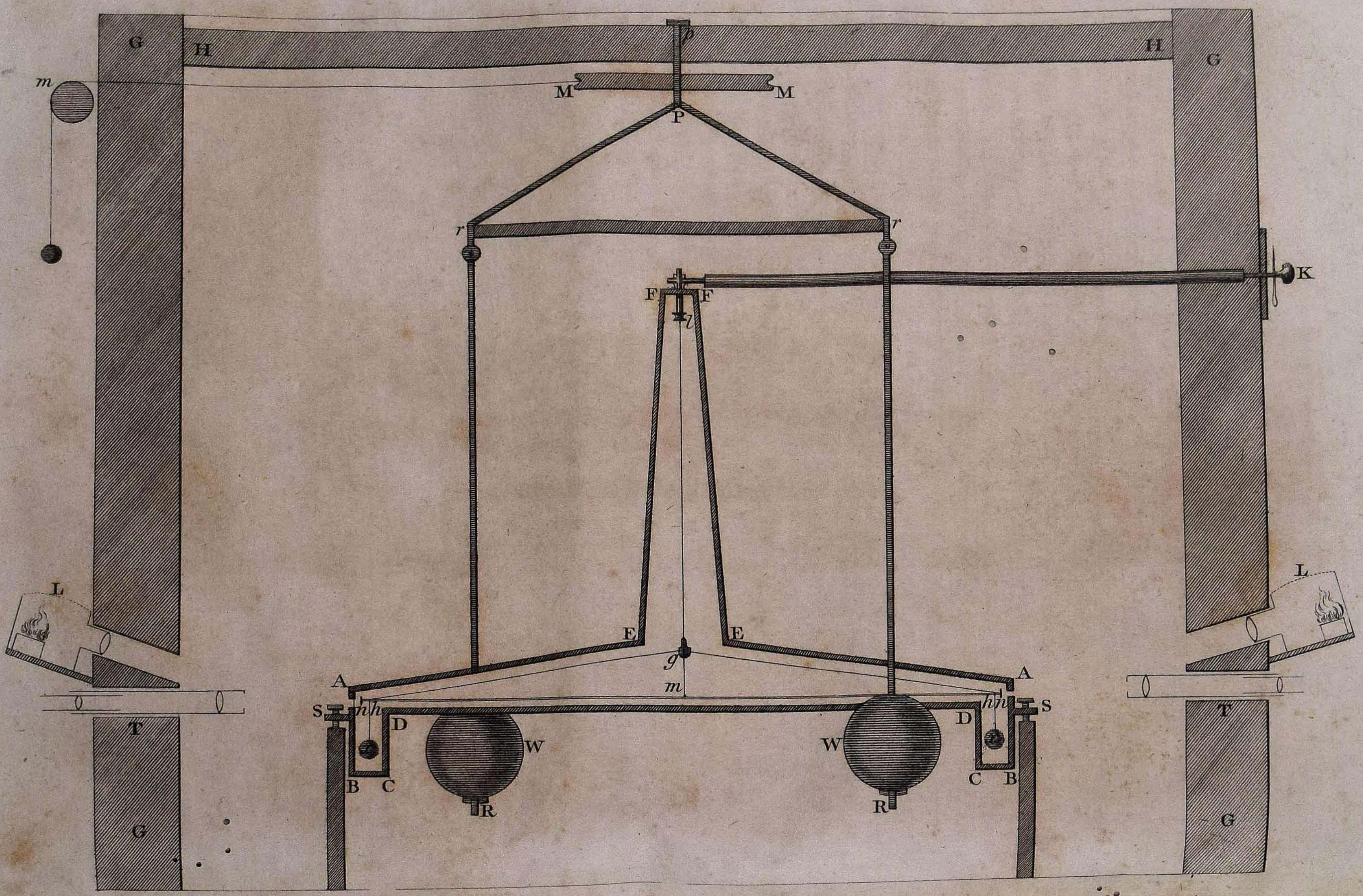
The laſt public meeting on the former buſineſs was held at the London Tavern on the 14th of laſt month (December); when it appeared by the Report that the ſubſcriptions amounted to upwards of ten thouſand pounds. The managing committee of ſubſcribers conſiſts of the Earl of Darnley, Lord Petre, the Hon. J. T. Townſhend, M. P. the Hon. Robert Petre, Sir William Geary, Bart. M. P., Mr. Ald. Luſhington, M. P., John J. Angerſtein, and Claude Scott, Eſqrs. Lieut. Col. Twiſs, Royal Engineers, Capt. Schank, R. N. T. Woodruffe Smith, Eſq., John Mavor, Eſq., Geo. Hawks, Eſq., and Benjamin Harriſon, Eſq.—Claude Scott, Eſq. is Treaſurer.

The undertaking will be begun and carried on by thoſe who ſhall firſt ſubſcribe to the propoſed ſhares, which are three hundred at a hundred pounds each; but the actual work will not be commenced till the whole ſum of thirty thouſand pounds ſhall have been ſubſcribed.

A fellow-labourer in the cauſe of ſcience, Mr. A. Tilloch, objects totally to the account I gave of Mr. Cartwright's apparatus for rendering the piſtons of ſteam-engines tight by metallic fittings, at page 365 of the preſent volume. As the whole of his remarks appear to me to be obviouſly erroneous, and the matter is before the public, I muſt be excuſed from entering into controverſy. He will himſelf perhaps, on ſecond thoughts, recollect that a circle, even though in diameter equal to that of the earth, will not be converted into a triangle by cutting it in three pieces; and that it is abſurd to ſuppoſe the third law of nature, that *action and re-action are equal and contrary*, can either be diſpenſed with or explained away. He will then probably look (not to the vacuum, but) to the lower plate of Mr. Cartwright's piſton for the re-action; which, by means of the piſton rod and the work required to be done, is made to act beneath the moveable pieces of the apparatus, while the ſteam preſſes their upper ſurface with no inconſiderable force; admitting its elaſticity to be equivalent to a ſingle atmoſphere only: a force ſufficient, in my apprehenſion, to prevent ground ſurfaces from ſliding freely, if at all, upon each other. But, as we are all liable to miſtakes in new practical matters, I could have wiſhed that Mr. Tilloch had reaſoned leſs diffuſely, and referred at once to the facts. If it be true that there was a ſteam-engine at work ſix months ago, when Mr. T. invited the public to inſpect it, at Mr. Rowley's, in Cleveland-ſtreet, Mary-lebone *; or if he can bring evidence that ſuch an engine has been at work for any conſiderable part of the time ſince he publiſhed his deſcription, and that the facility of operation, the power, and the durability of the apparatus are ſuch as he conceives them to be;—I cannot but think that he has, unfortunately, overlooked his beſt argument. To this argument, if offered, I muſt grant my aſſent; and I beg leave to aſſure him that I ſhall moſt readily attend, inſpect, and report concerning this engine, if he or the inventor will give me the opportunity, without in the leaſt regarding whether this part of my duty to the public ſhall confirm or overthrow thoſe opinions which the preſent ſtate of the facts has compelled me to give.

* I went there at the time, and ſaw the parts of a ſmall model or engine, which was not at work; and, upon late enquiry, I underſtand there is none there at preſent. I could gain no information whether there was any engine of this conſtruction at work, or in progreſs, at any other place.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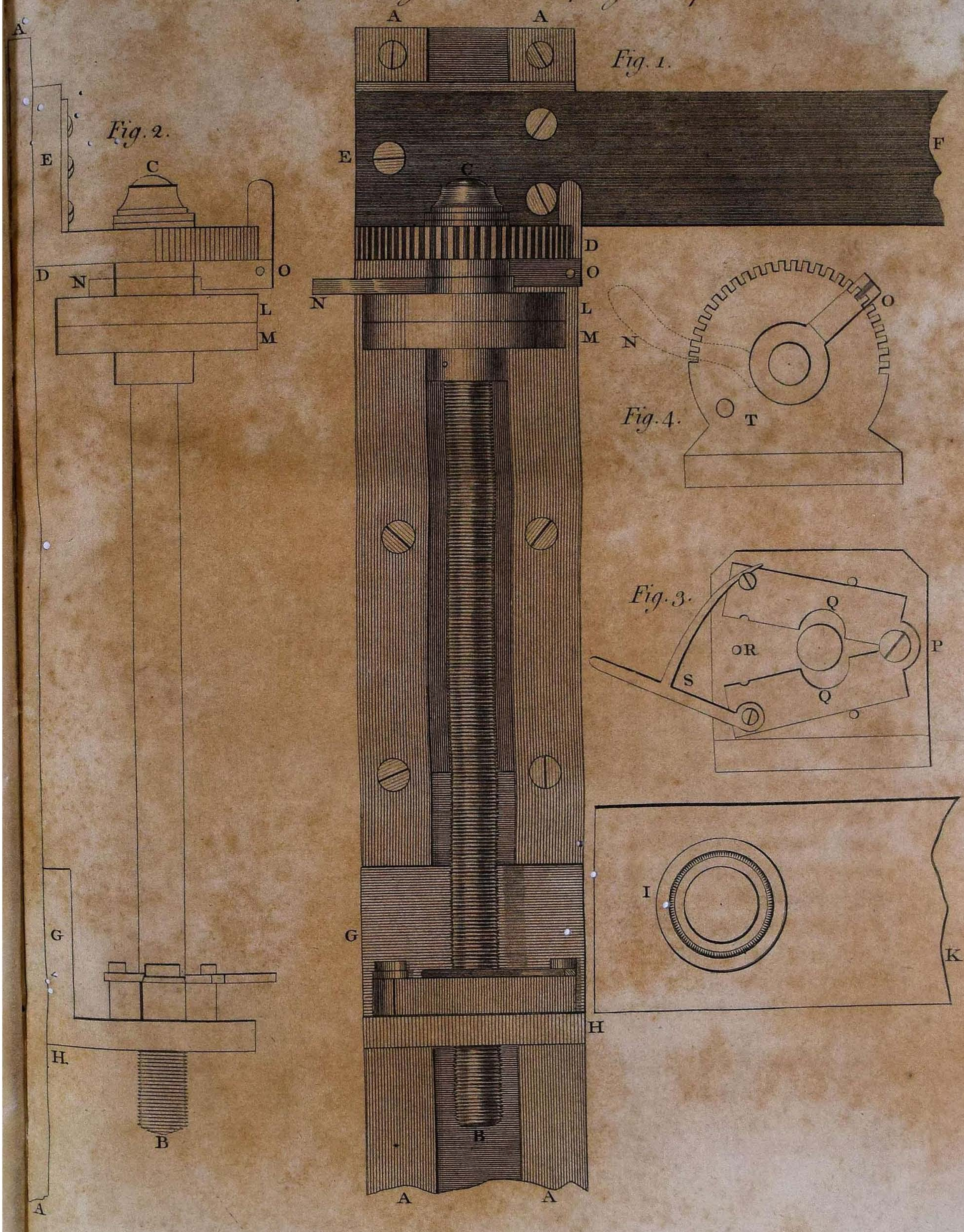

Barlow sculp.